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BY WILLIAM NICHOLSON.

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Of Foreign Works, Fl. Beaupoil; Count Apollos de Moussin Poushkin; Abbe Buée; Abbe Haüy; Romé de L'Isle; M. Maunoir; Huber; Professor Veau-de-Launay; Cit. Curadau; Michael de Grubbens; Professor Prevost; M. Seguin; M. Vauquelin; C. L. Morozzo; M. Parmentier; B. G. Sage; R. Prony; Bucholz; J. H. Hassenfratz; W. D'Hesinger and J. B. Berghius; Bode; Professor Proust; Tromsdorff; Strauss.

And of English Memoirs, abridged or extracted; Charles Hatchett, Esq. F. R. S.; Benjamin Count of Rumford, V. P. R. S. &c.; Thomas Young, M. D. F. R. S.; Mr. John Prior; Sir James Hall, Bart.; Mr. W. Bowler; Mr. Thomas Willis.

Of the Engravings the Subjects are, 1. Two Plates of Ap-paratus for Experiments on Heat; by Count Rumford. 2.

A new

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Soho Square, December, 1801.

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No. 37. H



A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

SEPTEMBER, 1804.

ARTICLE I.

*Letter from Mr. JOHN GOUGH, containing a Narrative of some
less common Effects of Lightning, by the Rev. JONATHAN WIL-
SON, and Remarks by the Communicator.*

SIR,

Middleshaw, Aug. 13, 1804.

THE following is a copy of a letter which I lately received from the Rev. Jonathan Wilton, Vicar of the parish of Biddulph, in Staffordshire. The facts and remarks contained in it, may prove acceptable to many of your philosophical readers, as well as to myself; and by giving it a place in your Journal, you will oblige

JOHN GOUGH.

Congleton, Aug. 1, 1804.

THE following circumstance induced me to defer my reply to your letter for a few days: On the 15th ult. I was informed in my way to church, by the farmer of Biddulph Hall, that, on the 7th, the lightning struck a plain arable field of his, which he uses at present for a cow pasture. I went with him, and found that the electric fluid had killed the tops of the taller thistles, in a circle of about twenty yards in diameter; but

Stroke of light-
ning in a plain
field, affecting a
circle of 20 yards
in diameter.

ON LIGHTNING.



The herbage and small thistles, no higher than the grass, did not appear to have been singed at all. Those in the centre, for about three yards, seemed much less injured than those nearer the circumference. In two places the soil was turned up, a few feet in length and an inch or two in depth; and where the ground ceased to be broken up, one might perceive that the lightning had glanced, with several ramifications, along the surface, but under the long grass, leaving a track, such as moles and mice sometimes make. In one place, where the impression made upon the soil was deepest, and somewhat resembled the letter V, a straight round hole appeared at the angle, which was about two feet deep, and about three inches in diameter.

Probability of a
stone having
fallen from the
atmosphere.

After I had left the place, it occurred to me that this hole might have been produced by a stone falling from the clouds, a phenomenon which has lately caused much speculation. The more I thought of this, the more probable it seemed. The hole was round and perpendicular, at no great distance from the end of the barn, and a tall tree was also near the place; either of which was more likely to attract the lightning than the plain field. A very lofty tower, which was struck a few years ago, was about three times the distance of the tree. The thunder was not so loud as when the tower was struck, but the smell of sulphur was much stronger. These considerations determined me to return the first opportunity and examine the place more thoroughly.

On examination
none was found.

From one cause or other I could not do this conveniently before yesterday evening, when the farmer and his son very obligingly went with me, and dug a circular pit about four feet in diameter, having the hole in its centre. At the depth of two feet they came at a shivery rock of grit, in which I hoped to find the expected stone imbedded; but when the soil was cleared away level with the bottom of the hole, there was no such thing; nothing appeared but small, oblong, flat pieces of gritstone, through the moist crevices of which the electric fluid probably escaped. Though disappointed in the principal part that I hoped to communicate, I resolved to send you the above, as I think the lightning very seldom descends so far into solid ground, unless when guided by some particular conductor. If the account afford you any information or amusement,

I shall

I shall be glad; and should the subject merit any of your queries, they will be received with pleasure by

Your humble servant,

JONATHAN WILSON.

Remarks on the above.

THE first part of Mr. Wilson's letter countenances Dr. Darwin's explanation of fairy-rings; for of all the thistles included in the electrified circle, those in the centre received the least injury. The hypothesis of Dr. Darwin has always seemed to me to labour under unfurmountable difficulties; and this is the reason why I point out a fact which establishes an exact agreement of the explanation, and natural appearances, in one instance.

Whether fairy-rings be thus formed.

Fairy-rings are permanent objects, the nature of which is but imperfectly understood; and it is to be wished that Mr. Wilson would find leisure to observe and describe the future consequences of the electric discharge at Biddulph Hall. The herbage of the ramified figure mentioned in the letter, will in all likelihood die in the course of a few weeks; because plants that have sustained strong electrical shocks, seldom survive the operation more than a month or two. Should this change take place in the circle, it will give a new appearance to the surface of the ground, by exposing to view the branched path of the lightning; nor is it improbable but that this alteration will be succeeded by another of a more singular kind. There is reason to suspect, that each ramification of the track will be again covered with a lively verdure the next spring, being accompanied at the same time by a contiguous patch of blasted herbage resembling its own figure. This suspicion is countenanced by certain observations of Dr. Hutton of Edinburgh, who remarked, that the fairy-rings upon Arthur's Seat annually increase in diameter; *i. e.* the withered circumference of each circle becomes green in spring, and is surrounded in a short time by a fresh ring of a russet colour. We have no right to dispute the justice of Dr. Hutton's observations, but the question is undetermined, whether the appearance is constant, or results from the nature of the soil. The perusal of the foregoing remarks will perhaps admonish my friend Mr. Wilson, that Fortune has furnished him with an opportunity

Comparison with Dr. Hutton's observations of the fairy-rings on Arthur's Seat.

of prosecuting the inquiry, such as she rarely affords to perform of correct observation. I will not trespass upon your pages by a comment on the remaining facts of my friend's letter; let them speak for themselves; in particular, the preference of a thunder cloud to low ground, in the presence of lofty objects, including a tall tree.

JOHN GOUGH.

ANNOTATION:

I BELIEVE the opinion that fairy-rings are caused by lightning, is of considerable antiquity. Dr. Priestley gave support to this conclusion by his experiment of the concentric rings formed on a polished metallic surface by the explosion of a battery. An effect of this kind produced in Kensington Gardens, is described, with an engraving, in our Quarto Series, Vol. I. p. 546.

W. N. G.

II.

On the Computation of Tables of Squares and Cubes. In a Letter from E. O.

To Mr. NICHOLSON.

SIR,

Computation of
squares and
cubes.

I SHOULD not have troubled you with any remarks on the method of computing squares and cubes, if I had not seen the paper which was printed on that subject in your Journal for last July. But as it appears, from that letter, that there may be persons who would like to employ themselves on these calculations, it is desirable that the plainest and easiest methods should be pointed out to them.

Every one who is accustomed to calculation, is acquainted with the advantage of constantly repeating the same operations. When the result of any calculation is to be made out, in some parts by multiplication and in others by division, subtraction, or addition, it is impossible for the most unwearied diligence to avoid occasional mistakes. It is right, therefore, to simplify as much as possible in this respect; and if the calculation

lation cannot be effected by a single operation, I believe I shall be joined by all persons accustomed to arithmetical computation, in recommending the use of addition* in preference to subtraction, and of multiplication in preference to division. Your correspondent H. G. does not seem to have paid sufficient attention to these considerations; and I fear that he not only would be soon wearied, if he were to work by the method which he recommends, but (what is of still more consequence) he could not depend for any continuance upon the accuracy of his computations.

The only method by which an extensive table like Mr. Councor's could have been calculated, must have been by the constant addition of differences; and the rules for this method may be easily deduced from the following considerations: Let $x+3a$, $x+2a$, $x+a$, &c. be any numbers in arithmetical progression. Then by the binomial theorem,

$(x+3a)^2 = x^2 + 6ax + 9a^2$	1st Differences.	2d Differences.
	$2ax + 5a^2$	
$(x+2a)^2 = x^2 + 4ax + 4a^2$	$2ax + 3a^2$	$2a^2$
$(x+a)^2 = x^2 + 2ax + a^2$	$2ax + a^2$	$2a^2$

It will be foreign from my present purpose to enter into a particular consideration of the manner in which these differences arise. It will be sufficient to remark, that the second difference is constant, and that its value will be always 2, when we consider a as $= 1$, or the progression $x+3a$, $x+2a$, &c. as a series of common numbers differing from one another by unity. Hence the first differences of the squares of such numbers become themselves an arithmetical progression, of which the common difference is 2; and consequently, if we know the first difference between the squares of any two such numbers, we can find the difference between the squares of the two next.

* If this opinion wanted any argument in its favour, I might support it by the authority of that able mathematician Captain Mendoza, who has calculated his new tables in such a manner as to make all the equations additional.

What

Computation of
squares and
cubes.

What has been said may be exemplified by applying it to the continuation of Mr. Councers's squares. For

798684121 is the square of 28261,

798627600 is the square of 28260,

56521 is their difference, therefore 56523 is the difference between the square of 28261 and the square of 28262. By the addition in every instance of 2, we may find the series of differences; and by the constant addition of them, we may easily find the squares. Thus

$$\begin{array}{r} 798684121 = \overline{28261}^2 \\ 56523 \end{array}$$

$$\begin{array}{r} 798740644 = \overline{28262}^2 \\ 56525 \end{array}$$

$$\begin{array}{r} 798797169 = \overline{28263}^2 \\ 56527 \end{array}$$

$$\begin{array}{r} 798853696 = \overline{28264}^2 \\ 56529 \end{array}$$

$$\begin{array}{r} 798910225 = \overline{28265}^2 \end{array}$$

By a similar method the table of cubes might be continued by the addition of differences; but, in this case, the calculation will be a little more complicated: Because, if the terms of any arithmetical progression be raised to the n th power, there will be n orders of differences, and the last will be the only one of them which will be constant. For the cube, therefore, where $n=3$, we shall have one order of differences more than for the square.

As before, let the progression be $1 + 3a, x + 2a, x + a, x$, &c.

COMPUTATION OF SQUARES AND CUBES.

Computation of
squares and
cubes.

$x+4a$	$= x^3 + 12ax^2 + 48a^2x + 64a^3$	1st Difference.	2d Difference.	3d Diff.
$x+3a$	$= x^3 + 9ax^2 + 27a^2x + 27a^3$	$3ax^2 + 21a^2x + 57a^3$	$6a^2x + 18a^3$	$6a^3$
$x+2a$	$= x^3 + 6ax^2 + 12a^2x + 8a^3$	$3a^2 + 15a^2x + 19a^3$	$6a^2x + 12a^3$	$6a^3$
$x+a$	$= x^3 + 3ax^2 + 3a^2x + a^3$	$3ax^2 + 9a^2x + 7a^3$	$6a^2x + 6a^3$	
x	$= x^3$	$3ax^2 + 3a^2x + a^3$		

If therefore we make $a=1$, the third difference will be constantly 6, and the second difference will become $6x+18$, $6x+12$, $6x+6$; so that if any three numbers be taken in an arithmetical series (in which $a=1$), the second difference of their cubes will be constantly six times the middle number.

For the sake of example, we may apply these observations to the continuation of Mr. Councor's cubes. We may consider 26559, 26560, 26561, as part of an arithmetical series: by the table we could find the difference of the cubes of 26559 and

Computation of
squares and
cubes.

and 26560, and, by what has been observed above, the second difference of the three cubes will be * 6. $26560^3 = 159360$; which, added to the difference between the cubes of 26559 and 26560, will give the difference between the cubes of 26560 and 26561. So far we should proceed exactly in the steps which are traced out by your correspondent H. G.; but I differ from him in this respect, that I should by no means recommend this method for the solitary calculation of any particular cube: it is only mentioned here for the sake of shewing how to ascertain the numbers by which we must commence the series of differences, when we want to continue a table which has already been calculated to a certain extent. For the third difference is constantly 6; therefore if we take an arithmetical progression, of which the terms differ from one another by that number, and which begins with 159360 (such as 159360, 159366, 159372, &c. &c.), we shall ascertain the second differences of the cubes which we want to find. These second differences must be added to the respective first differences, and, by that means, we shall find a series of numbers, which, added to each particular cube, will give us the cubes of the numbers next above it. Thus

$$\begin{array}{r}
 \overline{26560}^3 = 18736316416000 \\
 \overline{26559}^3 = 18734200194879 \\
 \hline
 2116221121 = \overline{26560}^3 - \overline{26559}^3 \\
 159360 \quad 2d \text{ Difference.} \\
 \hline
 2116380481 = \overline{26561}^3 - \overline{26560}^3 \\
 159366 \quad 2d \text{ Difference.} \\
 \hline
 2116539847 = \overline{26562}^3 - \overline{26561}^3 \\
 159372 \quad 2d \text{ D.} \\
 \hline
 2116699219 = \overline{26563}^3 - \overline{26562}^3 \\
 159378 \quad 2d \text{ D.} \\
 \hline
 2116858597 = \overline{26564}^3 - \overline{26563}^3 \\
 159384 \quad 2d \text{ D.} \\
 \hline
 2117017981 = \overline{26565}^3 - \overline{26564}^3
 \end{array}$$

Having thus ascertained the first differences, we may proceed to add them to the cubes;

* The dot (.) between the figures is here used as the sign of multiplication. N.

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Cubes.

$$18736316416000 = \overline{26560}^3$$

2116380481 1st Diff.

$$18738432796481 = \overline{26561}^3$$

2116539847 1st D.

$$18740549336328 = \overline{26562}^3$$

2116699219 1st D.

$$18742666035547 = \overline{26563}^3$$

2116858597 1st D.

$$18744782894144 = \overline{26564}^3$$

2117017981 1st D.

$$18746899912125 = \overline{26565}^3$$

The methods here stated are, probably, the easiest which can be devised for constructing an extensive table: but it must frequently happen, that the calculator will want the square or cube of some number which is greater than any which is contained in the table. It may be useful, therefore, to consider the assistance which the table may afford him in facilitating the computation.

It is well known, that $x^m \times y^m = (xy)^m$. * Therefore, if we want to find the square of a number which is double of any contained in the table, we have only to multiply the given square by 4. In the same manner, if we want to find the square of a number which is exactly three or four times as great as any contained in the table, we may find it by multiplying the given square by 9 or 16. Thus, for example,

$$16522331600 = \text{the square of } 128540;$$

4

$$66090126400 = \text{the square of } 257080;$$

$$16522274521 = \text{the square of } 128539;$$

$$149700470689 = \text{the square of } 385617.$$

* I do not know whether it is worth while to mention the circumstance, but your correspondent H. G. has made a mistake in the application of this rule. For he says that, if we "multiply the cube of any given root by 8, the product will be the cube of twice the next root:" whereas the product will be the cube of twice the given root.

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In this manner we may find the squares of all even numbers which do not exceed the double of the table: we may also find the squares of numbers, to a greater extent, which are multiples of 3: the multiples of 4 and 5 may be squared likewise, if we multiply by 16 and 25; but square numbers increase very rapidly, and consequently this method can be only used (with advantage) for the small multiples of the tabular numbers. We must, therefore, consider some other method which may be applicable to prime numbers and multiples of large ones.

The square of $2x + 1 = 4x^2 + 4x + 1 = 2x^2 + 4x + 2 + 2x^2 - 1 = 2 \cdot \overline{x+1}^2 + 2x^2 - 1$, or $2 \cdot \overline{x+1}^2 + x^2 - 1$. If, therefore, we want to find the square of an odd number which does not exceed the double of those which are squared in the table, we must divide the next even number below it by 2, and this half will be equivalent to x in the above equation; the next number above this half will give us $x + 1$. Having these two smaller numbers, we may find their squares by the table; and the sum of their squares multiplied by 2, will exceed the square required by 1. Thus, if it were required to find the square of $257079 = 2 \cdot 128539 + 1$: Here $x = 128539$, and $x + 1 = 128540$; therefore, by Mr. Cunn-
cer's table, the square of 128540 would be 16522531600
the square of 128539 would be 16522274521

$$\begin{array}{r} 33044806121 \\ 2 \\ \hline 66089612242 \end{array}$$

Therefore the square of 257079 is 66089612241.

Instead of examining each particular case, when the number to be squared is more than the double of those in the table, it will be best to consider the theorem, from which a general rule may be deduced. $\overline{nx+a}^2 = n^2x^2 + 2anx + a^2 = nx^2 + 2anx + na^2 + \overline{n^2-n} \cdot x^2 + \overline{1-n} \cdot a^2 = n \cdot \overline{x+a}^2 + n \cdot \overline{n-1} \cdot x^2 + \overline{1-n} \cdot a^2 = n \cdot \overline{x+a}^2 + \overline{n-1} \cdot x^2 + \overline{1-n} \cdot a^2$. Hence we must multiply x^2 by $n-1$, to the product add $\overline{x+a}^2$, multiply the sum of these two quantities by n , and this last product will exceed the square required by $\overline{n-1} \cdot a^2$. The only caution necessary, is in dividing the given number by x ; for as

we suppose it equal to $nx + a$, we should make x as great as it possibly can be, without being so large, that when added to the remainder a (as $x + a$), it should exceed the numbers of which the squares are given in the table. Because n and a are the only numbers by which we have to multiply, and as a must be less than n , we shall always have them as small as possible, if we take x according to the directions here given. If, for example, it be required to find the square of 385618, which is greater than twice, and less than three times 128540 (the greatest number which is squared by Mr. Cuncer's table); here $385618 = 3.128539 + 1$; therefore if $n = 3$, $x = 128539$, and $a = 1$, $x + a$ will be $= 128540$, and will be within the extent of the table: Hence

x^2 , or the square of 128539, would be by the table 16522274521

$$n-1, \text{ or } 3-1 \quad - \quad - \quad - \quad 2$$

$$33044549042$$

$(x+a)^2$, or the square of 128540, would be 16522531600

$$49567080642$$

$$n \text{ or } 3 \quad - \quad - \quad - \quad 3$$

$$148701241926$$

and as $n-1. a^2 = 2$, the square of 385618 will be 148701241924

If the square required were that of 385619, in this case $a=2$, and consequently, if $x = 128539$, $x+a$, or 128541, would exceed the extent of the table: therefore n must, in this case, be equal to 4, $x = 96404$, and $a = 3$, and

the square of 96404 = 9293731216

$$3$$

$$27881193648$$

the square of 96407 = 9294309649

$$37175503297$$

$$4$$

$$148702013188; \text{ but } n-1. a^2 =$$

3.9, or 27; therefore the }
square of 385619 is } 148702013161.

From this example we see the necessity of being careful that $x+a$ does not exceed the numbers in the table: at the same

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same time it may be remarked, that this is an extreme case, which can never occur but under particular circumstances: for it only happens when the number to be squared is less than a multiple of the highest number in the table, and greater than the same multiple of the number next less than the highest; that is, for a table like Mr. Councers's, it must be less than $m.128540$, and greater than $m.128539$: under these circumstances, if a be greater than 1, the case will occur which is the subject of the above caution.

It remains for us to consider the method of finding the cubes of numbers which exceed those given in the table.

Upon the principle to which we referred before, that $x^m \times y^m = (xy)^m$, we may find the cube of a number which is a multiple of any one contained in the table, by simple multiplication: for the cube of any number multiplied by 8, will give the cube of double that number; the cube multiplied by 29, 64, or 125, will give us the cube of 3, 4, or 5 times the number: Thus,

$$\begin{array}{r} 3368928644271 = \text{the cube of } 11991 \\ \hline 8 \end{array}$$

$$\begin{array}{r} 26951429154168 = \text{the cube of } 29982. \end{array}$$

$$\begin{array}{r} 1045678375000 = \text{the cube of } 10150 \\ \hline 27 \end{array}$$

$$\begin{array}{r} 7319748625000 \\ 2091356750000 \end{array}$$

$$\begin{array}{r} 28233316125000 = \text{the cube of } 30150. \end{array}$$

But as the cubes increase more rapidly even than the squares, it will still be more necessary in this case than in the former, to establish some means of finding the cubes of those high numbers which are either prime or not exactly a small multiple of a number contained in the table; and by proceeding in a manner similar to that which we used for the squares, we easily establish a general rule for this purpose: For $(n + a)^3 = n^3 + 3n^2a + 3na^2 + a^3 = n^3 + 3nan + 3na^2 + na^3 + n^3 - n \cdot a^3 + 3an^2 - 3an \cdot x^2 + a^3 - na^3 = n \cdot \overline{x+a}^3 + n \cdot \overline{n^2-1} \cdot x^3 + n \cdot \overline{n-1} \cdot 3ax^2 + 1-n \cdot a^3 = n \times \overline{x+a}^3 + \overline{n-1} \cdot \overline{n+1} x^3 + 3ax^2 + 1-n \cdot a^3$. Determine, therefore, n , x , and a , in the same manner

manner as for the square, and be equally careful that $x + a$ does not exceed the numbers in the table; add $n + 1 \cdot x^2$ to $3ax^2$; multiply the sum of these two quantities by $n - 1$; to the product add $x + a$, and multiply this sum by n : this last product will exceed the cube required by $n - 1 \cdot a^3$.

If, for example, it be required to find the cube of 53119, or $2.26559 + 1$: Then $n = 2$, $a = 1$, $x = 26559$, and $x + a = 26560$, and the cube of 53119 = $2 \times$

$26560^3 + 3 \cdot 26559^2 + 3 \cdot 26559 - 1$: Therefore, by Mr. Cuncer's table, the cube of 26559 would be 18734200194879

The square of 26559 - 705380481

18734905575360
3

The cube of 26560 - 18736316416000

74911033142080
2

therefore the cube of 53119 is - 149882066284160

Lastly, let it be required to find the cube of 79601, or $3.26533 + 2$: here $n = 3$, $a = 2$, $x = 26533$, $x + a = 26535$; therefore, by what has been demonstrated, 79601^3

= $3 \times 26533^3 + 2 \times 4 \cdot 26533^2 + 6 \cdot 26533 - 2 \cdot 8$.

$26533^3 = 18679234361437$
3

2112000267

• 37358468722874
2112000267

37360580723141
4

149142322492564
 $26533^3 = 18683458680375$

168125781572939
3

504377314718817

Therefore the cube of 79601 is - 501377314718801

E. O.

Analyf.s

III.

Analysis of a Triple Sulphuret of Lead, Antimony, and Copper, from Cornwall. By CHARLES HATCHET, Esq. F. R. S. From the Philosophical Transactions for 1804.

History of the mineral.

THE substance which forms the subject of this paper, has hitherto been regarded as an ore of antimony; it is extremely rare, and has only been obtained from Huel Boys, in the parish of Endellion, a mine which, from deficiency of profit, has for some time been abandoned.

The scarcity of the ore has probably been the cause of its being unknown to foreign mineralogists; indeed few even of the British cabinets possess it; but the most perfect and beautiful specimens are (as far as I know) to be seen in the splendid collection of Philip Rasleigh, Esq. of Menabilly, in Cornwall.

To Mr. Rasleigh we are indebted for the first description of this ore*; but no subsequent notice had been taken of it, until the preceding paper was written by the Count de Bournon, whose eminent merits, as a mineralogist and crystallographer, are well known to this Society.

I.

Sp. gravity.

The specific gravity of this substance is 5766, 65° Fahrenheit.

II.

Heat by the blow-pipe expelled sulphur and white fumes, and left sulphuret of lead and metallic copper.

If suddenly heated on charcoal, by the blowpipe, it crackles and splits; but, when gradually exposed to the flame, it liquifies, and, upon cooling, assumes a dull metallic grey colour.

When the globule was longer exposed to heat, white fumes (which at first had a sulphureous odour) were evolved, and partly settled on the charcoal.

Ebullition prevailed during the discharge of these white fumes; and the globule gradually suffered considerable diminution, remaining at length tranquil, and of a very dark gray colour.

* Specimens of British Minerals, selected from the Cabinet of Philip Rasleigh, Esq. F. R. S. &c. Part I. page 34, Plate XIX.

Upon

Upon examination, this appeared to be principally sulphuret of lead, which, like a crust, enveloped a minute globule of metallic copper, so malleable as to bear to be flattened by a hammer.

III.

Some of the ore, finely powdered, was put into a matrafs, and nitric acid diluted with an equal portion of water was poured on it. Upon being digested in a low heat, a considerable part, was dissolved, with much effervescence. Some sulphur, which floated, was separated; and the clear liquor, which was bluish green, was decanted from the residuum at the bottom of the vessel.

Digestion with dilute nitrous acid, &c. indicated sulphur, lead, copper, and antimony, with a little iron.

A great part of the excess of acid being expelled from the solution, it was largely diluted with distilled water, and some dissolved muriate of soda was added; but this did not produce any alteration in the transparency of the liquor. A solution of sulphate of soda was then poured in, and formed a very copious precipitate of sulphate of lead.

When this had been separated, the liquor was saturated with ammonia; by which it was changed to a deep blue colour. A few flocculi of iron were separated; and the remainder was found to contain nothing but copper.

The sulphur which had floated, was added to the residuum which had subsided to the bottom of the matrafs; and the whole was digested with muriatic acid. This solution was of a straw colour; and, when separated from the sulphur, and poured into a large quantity of water, afforded a plentiful white precipitate.

This precipitate was completely resolved into white fumes, by the blowpipe; and the muriatic solution of it, when added to water impregnated with hydro-sulphuret of ammonia, formed the orange coloured precipitate, commonly known by the appellation of golden sulphur of antimony.

IV.

Muriatic acid did not immediately act upon the pulverized ore; but a solution was speedily effected by the addition of a few drops of nitric acid: pure sulphur was separated; and the liquor, being decanted into water, yielded a copious precipitate of oxide of antimony.

The same results by treatment with muriatic acid and a little nitric.

The filtrated solution, by gradual evaporation, afforded crystals of muriate of lead; and the lead which afterwards remained in the liquor, was separated by a few drops of sulphuric acid.

The solution was now of a bright green colour, and, as before, was found only to contain copper, and a minute portion of iron; the former was therefore precipitated in the metallic state, by a plate of zinc.

These experiments, with others which I have not thought necessary to mention, prove, that the constituent parts of this ore are lead, antimony, copper, and a little iron, combined with sulphur; and, when the specific gravity, the external and internal colour, fracture, grain, and other characters are considered, there can be no doubt but that at least the three first metals exist in the ore, in, or nearly in, the metallic state, combined with sulphur, so as to form a triple sulphuret; to ascertain the proportions of which, the following analysis was made.

V. *Analysis.*

Analysis. Pul- A. 200 grains of the ore, reduced to a fine powder, were put
verizing. into a glass matrafs, and, two ounces of muriatic acid being added, the vessel was placed in a sand-bath. As this acid, even when heated, scarcely produced any effect, some nitric acid was gradually added, by drops, until a moderate effervescence began to appear.

Digestion with The whole was then digested in a gentle heat, during one
nitro-mur. acid, hour; and a green coloured solution was formed, whilst a quantity of sulphur floated on the surface, which was collected, and was again digested in another vessel, with half an ounce of muriatic acid.

left sulphur- The sulphur then appeared to be pure, and, being well washed and dried on bibulous paper, weighed 34 grains: it was afterwards burned in a porcelain cup, without leaving any other residuum than a slight dark stain.

The green solution afforded, by B. The green solution, by cooling, had deposited a white
dilution, oxide of saline sediment; but this disappeared upon the application of
antimony, heat, and the addition of the muriatic acid in which the sulphur had been digested.

The solution was perfectly transparent, and of a yellowish green: it was made to boil, and in this state was added to three
quarts

quarts of boiling distilled water, which immediately became like milk; this was poured on a very bibulous filter, so that the liquor passed through before it had time to cool; and the white precipitate thus collected, being well edulcorated with boiling water, and dried on a sand-bath, weighed 63 grains.

C. The washings were added to the filtrated liquor; and the whole was gradually evaporated at different times, between each of which it was suffered to cool, and remain undisturbed during several hours. A quantity of crystallized muriate of lead was thus obtained, until nearly the whole of the liquor was evaporated: to this last portion a few drops of sulphuric acid were added, and the evaporation was carried on to dryness; after which the residuum, being dissolved in boiling distilled water, left a small portion of sulphate of lead.

The crystallized muriate of lead was then dissolved in boiling water; and, being precipitated by sulphate of soda, was added to the former portion, was washed, dried on a sand-bath, and then weighed 120.20 grains.

D. The filtrated liquor was now of a pale bluish-green, which changed to deep blue, upon the addition of ammonia; some ochraceous flocculi were collected, and, when dry, were heated with wax in a porcelain crucible, by which they became completely attractable by the magnet, and weighed 2.40 grains.

E. The clear blue liquor was evaporated nearly to dryness; and, being boiled with strong lixivium of pure potash, until the whole was almost reduced to a dry mass, it was dissolved in boiling distilled water; and the black oxide of copper, being collected and washed on a filter, was completely dried, and weighed 32 grains.

200 grains of the ore, treated as here stated, afforded,

	Grains.	
A. Sulphur	34.	Product.
B. Oxide of antimony	63.	
C. Sulphate of lead	120.20	
D. Iron	2.40	
E. Black oxide of copper	32.	

But the metals composing this triple sulphuret are evidently in the metallic state; and white oxide of antimony precipitated

from muriatic acid by water, is to metallic antimony as 130 to 100; therefore, the 63 grains of the oxide must be estimated at 48.46, grains of the metal.

Again, fulphate of lead is to metallic lead as 141 to 100; therefore, 120.20 grains of the former are = 85.24 grains of the latter. And, lastly, black oxide of copper contains 20 per cent. of oxygen; consequently, 32 grains of the black oxide are = 25.60 grains of metallic copper.

Component
parts.

The proportions for 200 grains of the ore, will therefore be,

Sulphur	-	-	-	-	34.
Antimony	-	-	-	-	48.46
Lead	-	-	-	-	85.24
Iron	-	-	-	-	2.40
Copper	-	-	-	-	25.60
					<hr/>
					195.70
Loss					- 4.30.
					<hr/> <hr/>

Or, per cent.

Sulphur	-	-	-	-	17.
Antimony	-	-	-	-	24.23
Lead	-	-	-	-	42.62
Iron	-	-	-	-	1.20
Copper	-	-	-	-	12.80
					<hr/>
					97.85
Loss					- 2.15

These proportions, I have reason to believe, are tolerably exact; for I did not observe any essential variation in the results of two other analyses, which I made of this substance, with every possible precaution.

The loss may be principally ascribed to the oxide of antimony and sulphate of lead; but especially to the former, which has a great tendency to adhere to filters and glass vessels.

In some of the preliminary experiments, I obtained a small portion of zinc; but, having received, through the kindness of Mr. R. Phillips, of Lombard-street, some pure crystals of the ore, I found that the zinc had proceeded from blende, which was imperceptibly mixed in the specimens which I had first examined.

Description

IV.

Description of a compound Electrical Instrument for condensing and doubling: with Experiments. By Mr. WILSON.

London, August 10, 1804.

To Mr. NICHOLSON,

SIR,

I TAKE the liberty of troubling you with the following account and drawing of a compound condenser of electricity, which I think is an improvement on Cavallo's multiplier of electricity, described in his Treatise on Electricity in the third volume, of the fourth edition, and if you think it worth a place in your Philosophical Journal, you will very much oblige me by inserting it therein.

I am your obedient

Humble servant,

W. WILSON.

The drawing is an exact representation of the instrument, about half its real size, (Plate 3.) The plates A, B, C, D, and E are supported by glass sticks, covered with sealing wax at the upper part; but the plate F is supported by a wire, which has a joint at the bottom, by which it may be brought near the plate E, or thrown back away from it. 3 is a screw, which stops against the glass support of E, and regulates the distance of the two plates when one is brought near the other. The plates A, C and E are fixed to the bottom board, and B and D to the levers L I, which move round the pins p p, and are connected by the rod R. It will be seen by the drawing, that when the lever L is moved, the plates B and D are moved in contrary directions, that is, they both approach towards or recede from the plate C at the same time, but on opposite sides. The faces of the plates are ground flat, and they are so adjusted that if the screws 2 2 did not stop the levers when they are moved, the face of B would stop flat against the face of A, and so would the face of D against C. The screws 2 2 stop them when at a very small distance, which may be made more or less by turning the screws.

Its uses as a condenser, a single and double multiplier, and a doubler of electricity.

This instrument serves the purpose of a condenser, a single and double multiplier, and a doubler of electricity. When it is used as a single condenser, the plate B is brought as near A as the screw 2 will permit, and then the wires *b* and *f* touch one another and uninsulate B, which increases the capacity of A. As the capacity of A is increased it will receive a greater charge of electricity from any electrified body brought into contact with it. Consequently, when B is removed, the charge on A will be much more intense than it would have been if the plate B had not been opposed to it; but this intensity will not be greater than that of the electrified body, if its surface is not greater than the surface of the receiving plate A; for a condenser does not increase the quantity of electricity, it only collects it into a smaller space than it was before. So that if the electricity of a very small body is required to be ascertained, the simple condenser will not answer the purpose. Some other means must be used when this is the case, and Cavallo's multiplier is the least exceptionable of the instruments used for this purpose. The instrument represented in the drawing forms a multiplier, either single or double, for when B is near A and uninsulated, A will receive a much greater charge than it would if B had not been near. And because B is uninsulated, A will induce a contrary state on B, and of nearly equal intensity; which state is preserved: For the instant the lever is moved to carry B farther from A; the contact between the wires *b* and *f* is discontinued, and B insulated, and as it removes farther away from A, the intensity of its charge will increase the same as that of A will, and be ready to part with nearly the whole to another condenser.

Now as the plate B removes away from A, the plate D approaches C, and when at a small distance from it, the wires *d* and *g* come into contact, which operation uninsulates D, and therefore causes the two plates C and D to form a condenser; and at the same instant that *d* touches *g*, the wires *b* and *c* touch one another, so that nearly the whole of the charge of B is communicated to C, which induces a contrary state on D, as the charge of A did on B. D will therefore be in the same state as A is, which state is preserved; for the instant the plate B removes away from near C, the plate D also removes away from it, and the contact between the wires *d* and *g* is discontinued, and D insulated; and by continuing the motion the plate

plate B will come near A, and D will remove away from C till the wires *b* and *f* touch one another, at which instant the wire *d* touches the wire *e*. Now if the plate E and F are near one another, they will form a third condenser, and nearly the whole of the charge of D is communicated to E in the same manner the charge of B was communicated to C; at the same time B recruits its charge by coming near A, which will be communicated to C, which induces a contrary state on D, and which D communicates to E, and this operation is repeated every time the lever is moved backwards and forwards; so that the charge on C is increasing at every motion of the lever, while the charge on A remains the same; and when C becomes so much charged as not to be capable of receiving any more from B (which will be the case in a certain number of motions) the operation will still go on between D and E, the charge on E increasing while the charge on C remains the same, so that E will acquire a charge as much greater than C as the charge on C is greater than the charge on A, which will be manifested by removing the plate F away from E. When the wire *a* is screwed into the plate A, and connected with E by means of the wire 4 5; the instrument then possesses all the properties of the double of electricity, for then all the charge communicated to E (which is of the same nature as that of A) will be communicated to A, which will continually increase the intensity of its charge, and that will have an increased effect on B, &c. at every motion of the lever. There is no limits to this accumulation, but that where the charge is so intense as to pass from one plate to the other in the form of a spark.

I have made many experiments with this instrument relative to its spontaneous electricity, and I find, as a single multiplier (that is when an electrometer is connected with C) it has no effect on the most delicate gold leaf electrometer I could make; but as a double multiplier (that is when an electrometer is connected with E) there is some small effect if some electricity had been communicated to it within an hour or two, although it may have been discharged by touching each of the plates with a metal point (which I find is the most effectual way of discharging small portions of electricity,) but if it has stood three or four hours after being discharged, it will not give any signs of electricity. When used as a doubler it always becomes electrified with between eight and sixteen motions of the lever,

Its uses as a condenser, a single and double multiplier, and a doubler of electricity.

On the spontaneous electricity of this instrument.

On the spontaneous electricity of this instrument.

even though it has not been used for two or three months.— But if it has been used within two or three hours, the effect will take place with fewer motions of the lever, and if it has been used within a few minutes, two or three motions will be more than sufficient to ascertain the quality of the electricity. It is to be observed that the instrument was always discharged (by the metal point as before observed) between each trial.

In the course of making the experiments on the spontaneous electricity of this instrument I found that it was always positive if the instrument had not been used for two or three days, whatever electricity it was last charged with. But the time it must stand unused after it has been discharged, to produce this effect, depends a great deal on the weather; if the air is very humid, twenty-four hours is quite sufficient, but if it is very dry, it will require four or five days.

After I had observed that after the instrument was charged with positive electricity its spontaneous electricity was always positive; and that after it was charged with negative electricity, its spontaneous electricity was negative only within a certain time after it had been discharged, and then became positive; and also that it required a greater number of motions of the lever to produce a certain effect on the electrometer with negative electricity, the longer it stood after it had been charged with negative electricity, and that when it became positive, the longer it stood the less number of motions of the lever it required to effect the electrometer with positive electricity to a certain degree, and this within some certain limits. I was at a loss how to account for this change. However, after some consideration, I began to suspect that the plates (although all of the same metal, copper) had each a property peculiar to itself, of acquiring a certain small charge of one kind of electricity in preference to the other; and that if they were left to themselves they would naturally do so. I therefore began a set of experiments to ascertain the probability of this supposition. First, directly after the instrument had been used for negative electricity, I discharged it by touching each of the plates with a metal point, which I held in contact with each plate for two or three seconds. An electrometer was then connected with the plate A, and while it was in this situation, it was made to diverge with positive electricity;* it was then

* The electrometer was made to diverge by bringing an excited glass or sealing wax near it.

discharged by a slight touch with the finger, the lever was then moved backwards and forwards about fourteen times, and the electrometer diverged with negative electricity. This was repeated several times with the same effect. On the spontaneous electricity of this instrument.

The lever was then put in such a position that the plates could not be supposed to act as condensers, one pair more than another, and the whole was left untouched for twenty-four hours. It was then tried, and with twenty motions of the lever the electrometer diverged with positive electricity.

It was then left untouched for eighteen hours more, and on trial the electrometer diverged with positive electricity. The instrument was next discharged with the metal point as before, and the electrometer made to diverge with negative electricity, which was discharged by a slight touch of the finger. The lever was now worked, and with nine motions of the electrometer diverged with positive electricity. It was then discharged, and left untouched for sixteen hours, after which it diverged with positive electricity after a few motions of the lever. It was then left untouched for five days, after that time it gave positive electricity when the lever was worked. These experiments I have repeated a great many times with the same effect.

As it appeared from the foregoing experiments that the residuum on the electrometer was insufficient to overcome the effect of the residuum on the plates, I was induced to charge the whole instrument with the effect of the electricity, I would wish to communicate to it, and this I did by making the electrometer diverge with the desired electricity, and while it was divergent I began working the lever, to communicate the effect to all the plates. I charged the instrument with negative electricity first, and discharged it with the metal point. The lever was then worked, and in eight motions the electrometer diverged with negative electricity. It was then left untouched for five days, when eighteen motions of the lever made the electrometer diverge with positive electricity. This has been often repeated with the same effect.

The instrument was next charged with positive electricity, and discharged as before, after which, six motions of the lever made the electrometer diverge with positive electricity. It was left untouched for twenty-four hours, when the electrometer

On the spontaneous electricity of this instrument.

trometer diverged with positive electricity with twelve motions of the lever, and so it did after it had stood untouched for six days.

The probability of the foregoing supposition concerning the property of the plates acquiring a charge of one electricity in preference to the other, was more strengthened by the following experiment, which has been repeated several times. The instrument was charged with negative electricity, and then left without discharging for twenty-four hours, when sixteen motions of the lever made the electrometer diverge with positive electricity.

I make no doubt if the plates were of different metals this effect would be more striking, and that in some cases we should have the contrary electricity. I intend to construct one of these instruments in such a way that plates of any metal may be put on, and their effect tried. I think this a subject well worth pursuing as it may throw some light on some of the phenomena of electricity, which have their causes at present buried in obscurity.

V.

Note respecting the Suspension of Zinc in Hydrogen, and the consequent Ignition and Fusion of Platina Wire. By AMICUS.

To Mr. NICHOLSON.

DEAR SIR,

Ignition and fusion of platina in Cuthbertson's gazometer,

I EMPLOY a *gazometer* of a much more simple construction and much less expence than Lavoisier's, which that skilful and intelligent artist, *Cuthbertson*, invented at my entreaty, about ten years ago. It is described in your valuable Journal (quarto series, vol. II. p. 235.) The brass rod or the thick wire, which conveys the electric fire through the upper receiver, has about half an inch of platina wire appended, to hang just over the aperture through which the hydrogen gas rises at the bottom in the brass work. Zinc of any other sort of metal would either be melted or readily oxidized. But the other day, I was much surprized to see the platina on becoming as usual *red hot* from the flame of the hydrogen gas, melt into globules

as readily as copper or brass wire would have done. I soon perceived from the colour of the flame of the hydrogen and the deposit of *white clouds* on the inside of the glass, that the rapid solution of the finely powdered zinc used to afford the gas, that this metal was actually suspended or dissolved in the gas, and hence the zinc acting to the platina fused the latter. It may be perhaps useful to know this fact, which occurred during the public lecture.

—caused by the volatilization of zinc, &c.

Your's, as usual,

AMICUS.

July 2, 1804.

VI.

Description of a Blow-Pipe acting by the Pressure of Water, by the Abbé Melograni. In a Letter from Mr. G. B. GREENOUGH.

To Mr. NICHOLSON.

SIR,

No. 15, Parliament-Street,
August 13, 1804.

PERMIT me to make you acquainted with the principle of an instrument invented by the Abbé Melograni, and used by him at the Royal Mineralogical Collection at Naples, as a substitute for the blow-pipe. I regret that I cannot send you an accurate account of its construction, as I made no sketch at the time, and speak only from recollection.

Blow-pipe supplied by two vessels alternately changing their positions.

Two hollow glass globes of convenient size were connected together by two brass tubes laid along-side each other, one of them having a valve or stop cock, I am not sure which, at each end, and a side tube of the same metal going off from the middle at right angles. The frame was attached to the vertical tubes, so as to allow the globes to circumsolve each other. Let the lower globe be half filled with water and inverted; Then the water in A (Fig. 2. Plate IV.) running into B through the tube C, will force a constant stream of air into the tube D, and thence, the upper orifice being closed, into the side-tube E, at the mouth of which the candle is placed. When the water is nearly run out invert the globes.

If

If the principle of this instrument shall appear to you deserving of the public attention, it would give me pleasure to see it submitted to the readers of your excellent Journal. I have the honour to subscribe myself,

SIR,

Your obedient humble servant,

G. B. GREENOUGH.

VII.

Outlines of the Mineralogical Systems of Romé de Lisle and the Abbé Haüy; with Observations. By the Abbé Buée. Communicated by the Hon. ROBERT CLIFFORD.

To Mr. NICHOLSON.

SIR,

Outlines of the mineralogical systems of Romé de Lisle, and the Abbé Haüy.

WHEN the public mind appears at any time to direct itself towards a particular science (as it does at present towards mineralogy,) no person can feel the necessity of removing any erroneous opinions relating to that science, more forcibly than a gentleman who dedicates his time, as you do, Sir, to propagate correct science by means of your Journal. Actuated by the same sentiments, may I request the insertion of the enclosed paper, whose object is to remove several erroneous opinions relating to the mineralogical systems of Romé de L'Isle, and of the Abbé Haüy. It was written by the Abbé Buée in French for a friend of his, but on its being communicated to me by the author, I requested leave to send an English translation of it to you, thinking it might be acceptable to those who are fond of philosophical pursuits. Should your opinion coincide with mine, you will certainly afford it a place in one of your ensuing numbers.

And I remain, Sir,

Your very humble servant,

ROBERT CLIFFORD.

Welbeck-Street, July 13, 1804.

*A Letter from the Abbé Buée to Mr. —, on Mr. Romé de L'Isle's
and the Abbé Hauy's Theories of Crystallography.*

SIR,

IN consequence of your request, I send you the parallel of ^{Parallel between the theories of Romé de l'Isle and Hauy.} the two Theories of Crystallography, which seem to divide mineralogists in this country, those of Mr. Romé de L'Isle and of the Abbé Hauy. You are perfectly acquainted with the former theory, but nearly a stranger to the latter. Having lived for six and thirty years in habits of intimacy with the Abbé, I dwell with pleasure on his works, and will do my utmost to satisfy your curiosity.

To Mr. de L'Isle is due the merit of having called the at- ^{De l'Isle first exhibited order in crystallography.} tention of naturalists to that neglected branch of mineralogy, crystallography; of having discovered that that branch, though neglected, was perhaps the most interesting part of mineralogy, and the only part which could raise it to the dignity of a correct science; in short, of having discovered order, by numerous observations as ingenious as new, there, where a Cronstet, a Bergman, a Buffon, or a Kirwan could perceive nothing but confusion; and thus seemed to rescue nature from the charge of caprice, almost imputed to it because great mineralogists had neglected to study its unerring laws.

It was exclusively reserved to the Abbé Hauy to point out, ^{Hauy first explained the laws of crystallization.} to explain, and apply those laws. He demonstrated where De L'Isle affirmed. He discovered those *hidden facts*, which he has since shewn to be the mathematical consequences of facts observed by De L'Isle. If the latter furnished a part of the materials, the Abbé has augmented and employed them. The discoveries of these two writers, ^{force} me to subdivide crystallography into two distinct parts; descriptive and philosophical; and under these two heads I will rapidly describe the labours of each author.

Descriptive.—The most important part of Mr. de L'Isle's ^{Descriptive crystallography.} work consists in his crystallographical tables. In each of these ^{De l'Isle's tables.} tables (seven in number) he describes one of the principal forms assumed by crystals, and then delineates the different modifications of which that form is susceptible, by means of different truncations (*truncature*) as he calls them.

For elucidation, let us take a cube, the primitive form of ^{Modifications of crystals described by truncations.} the second table. A cube, it is known, has six faces, eight ^{solid} faces, and six edges. Instance the cube.

solid angles, and twelve edges. If the cube be truncated in a parallel to one of its faces, a rectangled parallelepipedon will be produced, and the equality of the faces will be destroyed.

If the eight solid angles of the cube be struck off, eight new faces will replace the eight solid angles, and in place of six sides we shall have fourteen. If the twelve edges be taken off, twelve new faces will succeed the straight lines, and the solid will have eighteen sides. Such are De L'Isle's *simple troncations*. They may be then combined with each other, and made more or less deep; hence an immense variety of new figures. But these new forms again may be truncated in the directions either of their faces, solid angles, or edges, and these new troncations more or less deep, called by De L'Isle *sur-troncatures*, may also be combined with each other. Here the forms must multiply to infinity, and their boundless numbers will soon bury the primitive cube in oblivion.

It must not be supposed that nature has furnished us with this infinite series of forms; indeed Mr. De L'Isle in his tables has only mentioned those he had observed, with some few additional supposititious figures, of which several have been since discovered to exist.

Account of
De L'Isle's seven
tables of crystals.

This ingenious naturalist has given us, as I have already said, seven crystallographical tables. In the 1st he describes the tetrahedron and its modifications; in the 2d the cube; in the 3d the rectangular octahedron; in the 4th the rhomboidal parallelepipedon; in the 5th the rhomboidal octahedron; in the 6th the dodecahedron, with triangular faces, and to each are subjoined their respective modifications. The object of the 7th table is to point out certain modifications of the octahedron and parallelepipedon, whether rectangular or rhomboidal. Plates accompany each table, where the figures are drawn, and in the observations and notes on them are to be found the measures of the principal angles.

They contain
general solids;
which are sub-
sequently applied
to the crystals
of nature, viz.
*salts, stones, and
metals.*

These crystallographical tables exhibit only general representations of solids, which Mr. de L'Isle in the course of his work applies to the different crystals which had already been discovered, and fallen within his observation. His work consists of three parts. In the first he treats of saline crystals; in the second of stoney (*pierreux*) crystals, and in the third of metallic crystals. Those of the first class are artificial, those of the two latter classes are natural crystals, and are subdivided into genera, species, and varieties.

When

When treating of a species or of a variety, he refers his reader to the table where the figure of that species or variety is to be found, and he then enumerates every thing relating to minerals assuming that crystalline form: But I cannot terminate this sketch better than by the following extract from the Abbé Hauy's treatise on mineralogy.

" In short Romé de L'Isle reduced the study of crystallography to principles more exact and more consistent with observation. He classed together, as much as he was able, crystals of the same nature. From among the different forms belonging to each species, he selected one which appeared to him to be the most proper, on account of its simplicity, for the primitive form, and then supposing it to be truncated in different manners, he deduced the other forms, and established a certain gradation or series of passages from the primitive form to that of polyhedrons which would scarcely appear to have any connection with it. To the descriptions and figures which he gave of the crystalline forms, he added the mechanical measurement of the principal angles, and he shewed (a most essential point) that these angles were constantly the same in each variety. In a word, his crystallography is the fruit of immense labour by its extent; almost entirely new in its object, and of great value for its utility." (Vol I. page 17.)

The Abbé Hauy in his treatise on mineralogy embraces a far greater extent than Mr. de L'Isle. His mineralogy is not only descriptive, but it is physical, chymical and geometrical. In the persuasion that a mineral cannot be well described, nor even in many cases recognized, unless its physical, chemical, and geometrical characters are clearly laid down, the Abbé never omits any one of those characters when ascertained, and exposes with the most scrupulous exactness every thing relating to them that observation has authenticated. He has bestowed particular attention to the electrical and magnetic phenomena, and has enriched the science with a multitude of new and curious observations. He attentively examined the property of double refraction, which several transparent minerals enjoy; and here again he has extended the boundaries of science. A few minerals were known to possess this property, and the Abbé has discovered it in several where it had never been furnished.

When we consider that writers on mineralogy have hitherto grounded their systems *exclusively*, some on the exterior characters,

His method of description.

Quotation exhibiting a summary of De L'Isle's labours.

The mineralogy of Hauy is not merely descriptive, but physical, chemical and geometrical.

Electricity. Magnetism.

Double refraction.

Hauy has excluded none of the habitudes,

or properties of
minerals.

acters, others on the chemical properties of minerals, and that the Abbé really has, pursuant to his plan, (see in the beginning of the volume of plates, *La distribution methodique des mineraux, par classes, ordres, Genres et especes*, the methodical distribution of minerals into classes, orders, genera, and species) united all that has hitherto been discovered on mineralogy, without falling into that confusion which has ever been imputed to other mineralogical writers, we are almost astonished at his success. "To class minerals, to furnish the means of discovering to which class, genus, and species a mineral under examination belongs," are the two great problems which the Abbé Hauy proposes for solution.

Classification,
chemical: in-
vestigation by
external charac-
ters.

He solves the first in following Bergman's method (founded on chemical properties) much improved by the immense progress which chemical analysis has made since the days of that great chemist. In the solution of the second he follows Werner's method (grounded on exterior characters) but corroborated by a multitude of new experiments, easily made and brought to a surprising degree of correctness by the Abbé's own labours on the forms of crystals. But I perceive that the immensity of matter contained in this treatise is leading me from that point which I had particularly in view, I mean crystallography.

Hauy's descrip-
tion of crystals,
1. by delineation.

In the description of crystals the Abbé employs three different means. 1st He draws their figure; he does not give crystallographical tables as Romé de L'Isle, which are only general properties, but draws separately each species and variety. Every form given in the plates has been examined by himself; he has calculated every angle, and nevertheless his plates contain one third more figures than De L'Isle's tables.

2. By symbolic
signs designating
the laws of their
production.

2^d He makes use of symbolic signs, than which nothing can be more simple, and were invented not to recall the form of the crystal to the mind, but the laws by which it has been produced. Yet I have met with persons, who were so accustomed to these signs, that at first sight of them they could immediately figure to themselves the form of the corresponding crystals. These signs can also be spoken, and much circumlocution in consequence avoided. 3^d A significant nomenclature subdivided into general and particular. The general is for the *minerals*, and comprises only substantives; the particular for the *crystals*, and is entirely composed of adjectives. He studiously avoided

3. By nomen-
clature.

introducing new names, and nevertheless has been obliged to introduce many, where new substances, names capable of giving false impressions, or others void of signification and unsupported by long usage, required it. He then substituted names taken from the Greek; a language, he says, that eminently enjoys the faculty of combining several words together, so as to form one representing concisely the object to be named.—The adjectives used in the nomenclature of the crystals also allude to some remarkable circumstance of the crystalline form.

I shall now proceed to philosophical crystallography, which might be called the philosophy of mineralogy. It does not consist in searching for the primary causes of phenomena, nothing can be less philosophical than such a research; primary causes will ever be beyond the reach of the human mind! The immortal Newton was the first to point out to us by the method followed in his admirable book of the Principia, that the only true philosophical way of treating a physical science, or of explaining a natural fact, was to demonstrate that it was the mathematical consequence of a general law, grounded on an aggregate of facts already observed and capable of correct calculation. If any one of these conditions are wanting, we immediately launch out into hypothesis, explanations become vague, and however much we may be persuaded of the truth of our assertions, we can acquire no certainty.

Philosophical
crystallography.

The true method
of explaining
physics is to shew
that the facts are
mathematically
deducible from a
general law.
Hypothesis.

Let us apply these principles to our two writers. De L'Isle, in declaring that the various forms observed in crystals of the same substance are only modifications of one constant primitive form, certainly announced a most important truth. It was a flash of genius; but in a philosophical enquiry, to prove it and not simply to say it, was the necessary step. On the first inspection of his crystallographical tables, a student is tempted to think that important truth demonstrated; but on a closer examination, the impression is done away. The same order pervades every table. By slight passages the student is led from the simpler to the more compound forms, and after every passage, is tempted to say; this can only be a modification of the primitive; then when the real crystals, and the figures of the tables are compared together, and all those of the same species (with a very few exceptions) are found in the same table, how easy it is to persuade ourselves that nature *must* operate by similar

The method of
Rome de l'Isle
shewn to be de-
ficient in cor-
rectness.

similar passages when producing the various forms of crystals; and the primitive of the table before us *must* be the primitive of the crystal under examination. In a word it is the most simple form; and first impressions greatly strengthen the illusion. If persuasion was the sole object of philosophy, De L'Isle would have been a powerful philosopher; but philosophy must convince, demonstrate, and wrest consent, however violently opposed. An enemy must not therefore be able to make use of the same arms, or adduce the same proofs to establish a contrary opinion. Nevertheless such would be ease with Mr. De L'Isle's tables and the application of them; For it is an incontestable fact, that by a series of arbitrary truncations we may pass insensibly from any given form to any other. Grounded on this principle, and seconded by Mr. de L'Isle's ingenuity, any form may become primitive, and any other deduced from it. Now as the combinations are infinite, a multitude of tables may be constructed, forms of the same species may be dispersed in different tables: the most simple of each table will be the primitive, therefore forms of the same species will have different primitives. But when by the same principle both sides of the question can be proved, nothing is proved.

and may be
employed to
establish falsehood.

On the most
simple form.

To say the most simple form must be the primitive is an illusion, for we know not what is the most simple for nature. With our feeble organs and confined senses we can form no judgment of *simple*, when the operations of nature are in question. Nature embraces the entire universe, her laws are simple; but the combinations made according to those laws are unbounded, therefore complicated.

The system of
truncations is
the most ob-
vious.

Let us not forget, however, that the idea of truncations, and the idea of taking the most simple form for the primitive, are so natural that they must have been the first to present themselves to the man who was opening the career: "Often, says the Abbé Hauy, (vol. I. page 14) a more compound form only differs from a more simple one by certain little faces which may be produced by sections, either at the solid angles, or on the edges of the simpler form," and in a note he says, "this was the observation which gave the celebrated Romé de L'Isle the idea of his system of truncations, that he might successively deduce from each other the different varieties of crystalline forms assumed by the same substance."

Mr.

Mr. De L'Isle terminates the introduction to his work by certain axioms, as he styles them, the 2d and 16th are as follows: De l'Isle's proposed axiom.

"II. Every angular polyhedron, or every crystallized substance is a *SALT* in the most extended acceptance of that term."

"XVI. Every saline substance whose constituent parts are perfectly saturated and combined affects the *cubic* form, or its inverse the *octahedron*; whereas the salts which are not neuter, or whose constituent parts are not exactly combined, affect either the *prismatic* or the *rhomboidal* forms."

I need scarcely observe that, to treat these axioms only as doubtful, would be treating them kindly. The other axioms are matters of fact, from which he draws no consequence, and indeed it would have been difficult for him to have drawn any.

The Abbé Hauy does not undertake to prove generally, that among the different crystalline forms of the same substance, one of them is the primitive; but he produces that primitive form from each crystal, which is always similar in similar substances. He demonstrates it analytically and synthetically; by an analysis which might be called *mineralogical analysis*, and pointed out by nature herself: By a synthesis hitherto the property of mathematicians, but here supported by the general laws which his analysis has revealed to him. The constant accord found between this synthesis, and daily observation is a proof of the exactness of his method. Hauy's exhibition of the primitive form in crystals, by analysis and synthesis.

Two facts were the foundation of his theory: 1. In all times jewellers and lapidaries have remarked that stones are more easily cut in some certain directions than in others. 2. Whoever has been in the habit of seeing natural crystals must have observed, that when their forms are well determined, they are always terminated by plane surfaces. Thus, says the Abbé, "those soft outlines, and that roundness so frequent in the animal and the vegetable kingdom, where they are inherent to the organization, and contribute even to the elegance of the forms, indicate on the contrary in minerals a want of perfection. The characteristic of true beauty in minerals is the straight line, and it was with truth that Romé de L'Isle declared that line to be the peculiar property of the mineral kingdom." The first fact suggested the mineralogical analysis, and the second furnished him with the laws on which he grounded his synthesis. Fundamental facts. 1. Crystals may be cleaved in certain directions only; 2. their boundaries are right-lined or plane.

Enquiries on the first Fact.

On the analysis
or subdivision
of crystals into
regularly formed
portions by
fissure.

1. All crystals that can be split by means of instruments, offer to the view, if split in certain directions, plane and smooth surfaces. If divided in other directions, the fracture is rugged. I use the word *split*, and not *stewed* or *cut*, as the sections of the crystal are not to be obtained by slow and continued efforts, but by sudden shocks. Patience, dexterity, and habit enabled the Abbé to split a great number of crystals; in all he discovered plane smooth surfaces when split in certain directions, but when in other directions, the fracture was always rugged and irregular. I request, Sir, your attention to this important fact, it is fundamental, and the more important, as several persons of much general information have neglected to attend to it, and, in consequence, have supposed the whole of this theory to be grounded on hypothesis. It would be equally erroneous to confound these sections of crystals with De L'Isle's troncations. The latter indeed warns his readers, that by the word *troncations* he wishes only to figure the appearance of the crystal examined. They are not therefore real, but only a means of warping the imagination to the exterior form of the crystal, and are by their nature only descriptive. The Abbé Haüy's sections are real, and are pointed out to the observer by the interior structure of the crystals; they are experimental.

It is experimental,
whereas the
troncations of
De l'Isle were
hypothetical.

The inclinations
of the faces are
constant.

2. The plane smooth surfaces obtained by the above method are respectively parallel to 3, 4 or 6 planes. The mutual inclination of these planes to each other are constant in crystals of the same substance, whatever may be the exterior form of the crystal. Native antimony, phosphate of lead, and quartz seem to shew an appearance of more than six planes, and the Abbé Haüy leans to the opinion of only five planes in some cases; but as these are exceptions to the general rule, and would only tend to complicate this statement, I shall take no further notice of them.

Explanation of
the figures and
formation of
crystals by the
method of Haüy.

Let us suppose the smooth surfaces to be only parallel to three planes, or in other words, that the substance will only split in three directions, in that case the sections can only produce a parallelepipedon, whose nature is determined by the mutual inclination of the planes to each other. If the planes are perpendicular, it will be rectangular, &c.

We next suppose the smooth surfaces to be parallel to four planes. Here a distinction arises, whether three of these planes have

have a common interfection or not; and it must be remembered that if the four planes have a common interfection, no solid can be produced, as they can neither bound nor include a space. If therefore three of the four planes have a common interfection, the splittings will produce either one hexahedral prism, or three parallelepipedons, which will be similar or dissimilar, according to the similarity or dissimilarity of inclination of the planes, or one triangular prism. On the contrary, if the four planes only intersect each other two and two, there will be produced either one octahedron, or four parallelepipedons, or one tetrahedron.

Explanation of the figures and formation of crystals by the method of Haüy.

Lastly, let us suppose the smooth surfaces to be parallel to six planes; then there arise an immense number of cases.— But we will for the present confine ourselves to the only case that has hitherto been observed in nature: Where the interfection of the planes is two and two, then we obtain either, 1. dodecahedron with pentagonal, quadrilateral or triangular sides, according to the sections made, or fifteen octahedrons, or twenty parallelepipedons, or fifteen tetrahedrons. It may be proper to observe here that though the sections parallel to the six planes may be clearly indicated, nevertheless it rarely happens they can all be executed, but it will suffice for the purposes of geometry that they be clearly indicated to render the consequences drawn from them mathematically correct.

Having laid down these premises, let us proceed to the dissection of a crystal of carbonate of lime (the *spath calcaire* of De L'Isle) whose primitive form is a rhomboid or a parallelepipedon bounded by rhombs. Hitherto sections have only been obtained in the three directions parallel to its sides. If these sections be directed so as to always pass through the center of two opposite sides, they will produce eight rhomboids equal to each other, and similar to the original one. The same operation may be repeated on each of these eight rhomboids, and continued so long as the substance remains carbonate of lime, that is to say, to be a combination of 55 parts of lime, 34 of carbonic acid, and 11 parts of water of crystallization:— (see Bergman.) But this division of the crystals into similar solids has a term, beyond which we should come to the smallest particles of the body, which could not be divided without chemical decomposition; that is to say, without an alteration

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in the proportions of lime, carbonic acid, and water. These last particles which are still rhomboids, are what the Abbé Haüy calls the *integrant particles* of the carbonate of lime. In the supposition therefore that a rhomboid of this substance can only be divided in three directions, by sections parallel to the sides, it is evident that the integrant particles must be similar rhomboids.

If a crystal can be divided by sections in more directions than three, what will be the form of the integrant particles? For example, in the phosphate of lime (the chrysolite of De L'Isle) where the sections are parallel to four planes, three of which have a common intersection. According to what has been said above, these sections can produce either one hexahedral prism, or three parallelepipeds, or one triangular prism. It is evident that by carrying the division, according to those sections, to its greatest length, either the last hexahedral prism, or the last three parallelepipeds, or the last triangular prism, will be produced. Are these last solids the integrant particles; are each of them so; or is there only one of them entitled to that denomination; and if only one, which of them? My answer is, only one of them; and that one, the triangular prism, which may be proved thus.

It cannot be denied that the integrant particle is that little solid which contains the least possible quantity of the body, without affecting the chemical composition of the substance. This granted, let us suppose the hexahedral prism to be the integrant particle. In that supposition the last triangular prism must contain the last hexahedral prism, and is equal to the latter more three little triangular prisms, or in other words to nine similar triangular prisms, while the hexahedral prism only contains six. But the last triangular prism and the last hexahedral prism each contain an exact proportion, and therefore a similar proportion of chemical component parts; therefore their difference also contain an exact proportion; but it is impossible to conceive how their differences can contain the exact proportion, unless each of the three little triangular prisms also contain it, they must therefore contain it, and each of them must be an integrant particle; therefore the hexahedral prism cannot be one; neither can the parallelepipeds be integrant particles, as the same arguments will stand good against them which have been applied to the hexahedral prism; therefore

fore the triangular prism must be the integrant particle: * Explanation of the figures and formation of crystals by the method of Haüy.
 “ The forms of the integrant particles, says the Abbé (vol. I. page 30) may be reduced to three, the tetrahedron or the most simple of pyramids, the triangular prism or the most simple of prisms, and the parallelipipedon or the most simple of solids, having parallel sides two and two, and as four sides are necessary to circumscribe a space, it is evident that the above three forms in which the number of sides are successively four, five and six, are again in this point of view the most simple possible.”

The phosphate of lime or chrysolite is a substance that has given rise to much curious anecdote. It shews in what a state Abbé Haüy found the mineralogical nomenclature, and points out the accuracy of his analytical method. Achard, a chemist at Berlin had analysed the chrysolite, and published that it contained, of silica 15 parts, alumina 64, lime 17, and of iron one. This startled the celebrated Vauquelin, who had seen Klaproth's analysis of the chrysolite (the apatite of Werner) containing of lime 55 parts, and of phosphoric acid 45, (probably the water of crystallization is added to the acid.) A Frenchman of the name of Launoy sent a quantity of this substance to Paris, some of it was purchased by the *Ecole des Mines*, and Vauquelin was desired to analyse it. The latter soon suspected Achard had been misled by the name, and had not obtained the proper substance, a mistake the more easily made as, says Vauquelin, “ the name of chrysolite was given to a great variety of stones, such as the *peridot*, the *chrysoberil*, the *olivine*, (since found to be the same as the *peridot*), and in general to stones having a yellow colour.”

He soon discovered the chrysolite sent from Spain contained lime and phosphoric acid. “ I had no sooner made this discovery, says he, than I enquired of Abbé Haüy whether he had compared the integrant particles of the chrysolite with those of the apatite or crystallized phosphate of lime. He answered me that he had not made the comparison, but that he would get his papers on primitive forms, (this was four years before the publication of his work) see what notes he had

* Therefore the sections producing the hexahedral prism cannot lead to the integrant particle: therefore all sections, though perfectly practicable in crystals, will not lead to the integrant particle.
 made

Explanation of
the figures and
formation of
crystals by the
method of Haüy.

made on each of those substances, and immediately compare them together; when with pleasure he found that there was not the least variation between them. Thus had the Abbé Haüy discovered by the help of geometry alone, that which was confirmed by chemical analysis, and this satisfactory accord between two sciences apparently so distant from each other, while securing each others steps, serves also to shew the certainty of the principles on which they are grounded."

Journal des Mines, xxxvii. page 21.

A more singular anecdote is what took place with respect to the emerald and the beril. Vauquelin had analysed the emerald of Prerou, and read the result of it, at a sitting of the *Ecole des Mines*, which is preserved in the Journal, No. XXXVIII. page 96: viz. of silica 64, of alumina 29, oxide of chroma 3, of lime one, and of volatile substance 2; I have neglected decimals. Soon after he discovered a new earth, which he called the Glucine, and gives the following account of it to the National Institute: "The Abbé Haüy having observed a perfect conformity between the structure, the hardness, and the weight of the beril and the emerald, pressed me a few months back to make an analysis of these two substances, to know whether they contained the same principles, and in similar proportions. In the result, the fact that will most interest the Institute is the discovery of a new earth, &c. &c." *Annales de Chimie*, vol. XXVI. p. 157.

I am certain, Sir, that it will give you no less pleasure to learn that Vauquelin, in consequence of this discovery, made an addition to the paper read at the *Ecoles des Mines*, beginning thus, "Since the reading of the above paper, having discovered a new earth in the beril, and as this stone, according to the observations made by Haüy, contained substances similar to the emerald, I have in that point of view made a new analysis of this latter stone." And the former analysis was immediately corrected, and the 29 parts of alumina became 16 parts of alumina and 13 of a new earth. I hope, Sir, it is not too much to say that, on this occasion a new earth was discovered, if not by, at least in consequence of, a geometrical analysis.

But to return to our subject, the Abbé makes a distinction between the *integrant particle* and the *primitive form*. The former, as I have said, is that last particle, which preserving an exact proportion of the component parts, contains the least
number

number of those parts; it is the last term of mineralogical analysis. The primitive form, on the contrary, is its first result, and retaining the exact proportion of the component parts, contains the *greatest number* of those parts. It is easy to see that in the case above mentioned of the phosphate of lime, the hexahedral prism will be the primitive form, precisely for the reasons adduced to shew that it is not the integrant particle. Though the Abbé does not decidedly define the integrant particle as containing the *minimum* of space under the *maximum* of surface, and the primitive form as containing the *maximum* of space under the *minimum* of surface,* nevertheless he makes a remark that authorises the above definitions (which, Sir, you will observe are mine, lest any fault be found with them.) He says the dodecahedron with rhomboidal sides, which is the primitive form of the garnet (grenat) contains the *maximum* of space under the *minimum* of surface; and if it be cut into two equal and similar parts, it will present the same form as the bottom of the cell of the honey-comb, which has the similar property.

Explanation of the figures and formation of crystals by the method of Haüy.

An objection might be taken on the *cuivre pyriteux*, and the *cuivre gris*, or the yellow and grey copper ore of Kirwan, the Abbé mentioning the regular tetrahedron as their primitive form, and not the octahedron as in other cases. The reason may be, that all the crystalline forms of their substances which he describes are slight modifications of the regular tetrahedron.

"The primitive forms hitherto observed, says the Abbé Haüy, are reduced to six.—The parallelepipedon, the octahedron, the tetrahedron, the regular hexahedral prism, the dodecahedron bounded by rhombs all equal and similar, and the dodecahedron with triangular sides, formed by two right pyramids united base to base."

He also makes a distinction between integrant particles and *subordinate particles*; these latter are always parallelepipedons. I shall speedily mention whence they derive their name. They are substituted for the integrant particles, to facilitate calculations, and it is worthy of observation that the parallelepipedon can always be obtained in all dissections of crystals. Thus far, Sir, I have stated the first principles of mineralogical analysis; I shall now proceed to the synthesis.

(To be continued.)

* The inclination of the intersecting planes being the same.

VIII.

Description of a Filtering Machine invented by Professor Parrot of Paris. In a Letter from a Correspondent.

To Mr. NICHOLSON.

SIR,

Filtering machine by sand in a reversed tube.

I ENCLOSE you a drawing of a filtering machine invented by Professor Parrot, of Paris, which for simplicity and utility seems superior to any other I have met with. You will perceive that from the curvature of its form, it purifies the water both by descent and ascent, and is, consequently, a closer imitation of the operations of nature than those in which the water penetrates in but one direction. Among the advantages which he ascribes to it, he instances the "prolongation of the stratum of sand, which does not considerably diminish the product of the filtre, but contributes remarkably to the purity of the fluid," and that "the difference of its water-level has an essential influence on the quantity of purified water obtained in a given time;" he therefore recommends an apparatus of eighteen inches long from A to D, two inches thick, and four broad, which, he says, will yield six (Paris) pints of pure water every hour: a machine of this size requires only a difference of two or three inches in the height of the water.

I am, Sir,

Your constant reader,

London, August 20, 1894.

A. A.

a. *Description of the Machine.*

The reservoir G. (Fig. 1, Plate IV.) may be of any form or dimension which is convenient; the principal part of the machinery consisting of a square vessel bent in the form of an inverted syphon. The curve may be circular, elliptic, or in any other direction. This vessel is to be filled with fine pure sand to nearly the height of the dotted line *xy*, which denotes the ascent of the water to D, whence it flows into the receiver. To the part marked A B, which must always be above this line, a woollen bag is attached, open at the top and reaching to the sand: this collects the coarsest impurities, and prevents the sand from becoming foul for a longer time. In large machines

chimes a water-tight trap-door may be made at F, for the purpose of removing the sand when it is overcharged with impurities. The small diameter of the machine from which the drawing was taken, was eight inches from B to E: the perpendicular height from C to A B was eight inches and three-fourths, and from C to D four inches and one-twelfth.

IX.

Medico-Chemical Researches on the Virtues and Principles of Cantharides. By H. BEAUPOIL.

(Concluded from Page 71 of Vol. VIII.)

HE also proved that the black precipitate easily became dry, brittle, and friable in the air; that it reddened the tincture of turnsole; that it combined very readily with potash, disengaging ammonia; that, when distilled by an open fire, it swelled and yielded an acid liquor, a thick oil, and carbonate of ammonia; and that it left a dry, shining, friable coal in the retort.

Experiments and
observations on
cantharides.

Proceeding afterwards to the examination of the yellow matter remaining in solution in the alcohol, Cit. Beaupoil informs us, that, when it is concentrated by the evaporation of its solvent, it retains the same odour and the same taste as the extract; that it is completely dissolved in water, and reddens the tincture of turnsole; that it combines entirely with potash, without any disengagement of ammonia; and that the result of this combination is an homogeneous and glutinous body, soluble in water and precipitable by a weak acid; finally, that, distilled by an open fire, it swells very little, yields an acid liquor, a black and fetid oil, and carbonate of ammonia; but that, in general, all these products are in smaller quantity than in those obtained from the black precipitate.

Among these different results the author thought it necessary to direct his attention more particularly to the acid, which, as has been seen, manifests itself so readily in the infusion of cantharides, or in the extract which they afford.

At first he was of opinion that this acid was analogous to that of vinegar; he also thought that its existence might be attributed

Experiments and
observations on
cantharides.

buted to the custom prevalent with those who collect the cantharides, of exposing them to the vapour of this acid; but when he found, on subjecting some of these insects which had been procured without the assistance of vinegar to experiment, that they were similar to those of commerce, he was obliged to renounce his first idea, and to endeavour to ascertain the nature of the acid they offered him, by experiment. It appears that his endeavours have, in this instance, been unsuccessful; for he finishes by inferring that his progress is not sufficiently advanced to determine with certainty; and that, although the acid in question has some analogy with the phosphoric, he, nevertheless, does not think that it possesses all its properties, and, consequently, is of opinion it should be considered as a peculiar species, until new experiments have shewn that to which it in reality belongs.

The third product of cantharides, called by Thouvenel and by Cit. Beaupoil, green matter, does not seem to experience any change from the air, at least in its physical properties. It is insoluble in cold water; it liquefies in warm water, floating on its surface like an oil; alcohol and ether dissolve it, and its solution in these two menstrua is decomposed by water. Oxigenated muriatic acid brought into contact with this matter, and renewed from time to time, at first seems not to have any action on it, but at length small whitish, brilliant particles are detached from it, which fall to the bottom of the vessel: in less than a week it loses its smell and its colour, and becomes thick and glutinous; and notwithstanding repeated washings, it constantly retains the odour of the oxigenated muriatic acid.

Diluted nitric acid, assisted by heat, gives it a ruddy colour, a rancid, penetrating odour, and also a considerable consistence.

Caustic soda combines with it without the aid of heat, and without a disengagement of ammonia. The product of this union is decomposed by the acids.

Exposed to an elevated heat it fuses, and forms a liquid of an oily appearance and slightly transparent, which, by cooling, quickly resumes the solid state. By a more powerful heat it is decomposed, its colour changes, a yellowish oil, very analogous to that obtained from the distillation of wax, and an acid phlegm, pass into the receivers, but not an atom of carbonate of ammonia.

With

With respect to the parenchyma forming the residue of the different macerations, infusions, and decoctions in water, alcohol, and ether, the author, after having ascertained that these fluids were incapable of extracting any thing more, treated it with caustic potash, which immediately caused a disengagement of a very sensible ammoniacal odour. When this odour was dissipated, the liquor was filtered, and instantly mixed with muriatic acid: the mixture became turbid, and gradually yielded a precipitate, which, dried and thrown on burning coals, exhaled an odour similar to that of animal matters in combustion.

Experiments and
observations on
cantharides.

Distilled in a retort, this parenchyma yielded phlegm, a dense empyreumatic oil, and a considerable quantity of carbonate of ammonia. The residue of the distillation was a species of coal, from which a white ash was obtained by incineration in the open air, in which were found carbonate of lime, calcareous phosphate, sulphate and muriate of lime, and, finally, oxide of iron.

In recapitulating the quantities of each of the products obtained by means of the experiments which have been cited, the author asserts that one ounce of cantharides, well dried, contains nearly

Black matter	-	1 gros 2 grains ;
Yellow matter	-	1 — 2
Green matter	-	1 — 8
Parenchyma	-	4 — 36
Acid	-	An indeterminate quantity ;
Calcareous phosphate		12 grains ;
Carbonate of lime	-	2 .
Sulphate and muriate of lime		4 .
Oxide of iron	-	2 .

To complete the work which Cit. Beaupoil had undertaken, it remained for him to determine the physiological properties of cantharides, as well as those of the most essential of their immediate materials, and he appears to have executed this with success in the fourth part of his dissertation. Among other things, it results from the different experiments which he has made on this subject,

1st, That cantharides which have not undergone any preparation, almost always produce disagreeable effects when taken

Experiments and taken internally; but that these effects, with respect to their
observations on intensity, are proportionate to the age, the strength, and the
cantharides. constitution of the animals, and to the dose which has been
administered to them: That the œsophagus, the stomach, and
the smaller intestines, are the parts which are principally af-
fected: that those animals which are not overcome by it, ex-
perience a desire to vomit, very considerable pains and vari-
ous affections, which seem plainly to indicate that the parts
which have been touched by the cantharides, have a sort of
tendency to be disorganized.

2d. That the aqueous extract of cantharides, in smaller
doses than the insects themselves, produces nearly the same
effects as they do; and also, that its action on the urinary pas-
sages is very marked.

3d. That the black matter is much less active than the ex-
tract; that the animals to which it has been given, are only
affected by gripings and vomitings, and very rarely are killed
by it.

4th. That the green matter given internally does not appear
to have deleterious qualities, since all the animals to which
even strong doses had been administered, did not seem to be
affected uneasily by it.

5th. That the yellow matter does not seem to be more ac-
tive than the green matter.

6th. That the extract, the yellow matter, and the black
matter, applied separately to the surface of the body, occa-
sioned vesication in nearly the same space of time.

7th. That the green matter, applied externally, does not
seem to act when alone; but that its action is speedily mani-
fested when it is divided by wax, and by that means receives
the confidence of a cerate.

I must not omit to mention, that Cit. Beanpoil was not
satisfied with experiments made on animals, but had the cou-
rage to repeat them on himself. It was from having obtained
the information he sought in this manner, that he considered
himself intitled to conclude, that the vesicating property resides
particularly in the extractive part and in the green part of the
cantharides, but that the extractive part alone acts on the
urinary and genital system.

It will be obvious from the details I have given, that the
author has carried the examination of cantharides farther than

Thouvenel.

Thouvenel. But although his work is greatly extended, it nevertheless is not yet complete, since much remains to be done, particularly with respect to the green matter; for it is difficult to conceive how it should have no action on the animal economy when administered internally, since, applied externally, it produces a vesicating effect. This objection which I have made to the author, the importance of which he acknowledges, will doubtless be one of the motives which will determine him to renew his experiments to remove doubts, and to shew more clearly what is to be expected from the employment of the different parts composing an ingredient from which medicine has received such great benefits.

X.

Experimental Determinations of the Latent Heat of Spermaceti, Bees' Wax, Tin, Bismuth, Lead, Zinc, and Sulphur. By Mr. WILLIAM IRVINE. Communicated by the Author.

Bedford Street, Aug. 24, 1804.

IT will scarcely be denied that the discovery of the existence of latent heat in all fluid and vaporous bodies, is one of the most curious and important hitherto developed in the progress of chemical philosophy. The merit of first investigating this subject is universally attributed to the celebrated Black. By a few simple and clear experiments he demonstrated, that, before any portion of ice can become water, it must receive or absorb as much heat as would have raised the temperature of an equal quantity of water by 140° . By other experiments, in some of which Dr. Black was assisted by my father and Mr. Watt, it was proved in a manner equally satisfactory, that water cannot be converted into steam unless it admit a quantity of heat sufficient to have heated the water 8 or 900° . Having proceeded so far by experiment, Dr. Black made a general inference, and extended his theory to all cases of fusion and vaporization whatever.

The only other philosopher, as far as I know, who has attempted to determine the exact quantity of the latent heat of other bodies besides water, was Dr. Irvine. Landriani made some

Experiments and observations on cantharides.

Discovery of latent heat by Dr. Black.

Dr. Irvine's examination of the latent heat of other bodies besides water.

Spermaceti
 145°, bees' wax
 175°, tin 500°.

some experiments to prove that the fluidity of alum, sulphur, and some metals, was accompanied with an absorption of latent heat; but I believe he made no attempts to ascertain the precise quantity. In Dr. Black's lectures we are informed, that Dr. Irvine found the latent heat of spermaceti to be 145°, of bees' wax to be 175°, and of tin to be 500°. From the very imperfect notes which I possess of the methods used to determine the two former, I believe that the 145° are measured by the capacity of fluid spermaceti, and the 175° by that of fluid wax: But of this subject I will take another opportunity to treat more amply. I am perfectly ignorant of every circumstance regarding the latent heat of tin as determined by my father, having been unable to find any notes of his experiments for determining this point.

Experiments
 made by the
 author.

During the course of the present year I have turned my attention a little to this subject, I mean to the investigation of the quantity of latent heat necessary for the fusion of various bodies. I was persuaded that an addition to our knowledge of latent heat would at least increase the store of facts, and might perhaps give rise to some improvement or correction of theory. The vessels which I employed in all my experiments were Florence flasks, of which the neck was cut off. In these the water made use of was contained, and the vessel was supported on a slight wooden stand, which presented a very small surface to abstract heat from the materials. The orifice at the superior part of the vessel, was in general not more than sufficient to permit the ready introduction of the fluid examined; probably from an inch and a quarter in diameter to a little more, so that a very small surface of the water was exposed to the air. The weight of the glass was in all cases previously ascertained. These circumstances being premised, I proceeded as follows:

The vessels.

Determination
 of the latent heat
 of fluid bismuth
 (or the number
 of degrees
 through which
 any quantity of
 bismuth would,
 by the heat it
 gives out in con-
 gelation, raise an
 equal quantity of
 bismuth, from
 beneath the
 freezing point of
 the metal).

The first substance which I submitted to examination was bismuth. The melting point of this metal was, by the thermometer which I used, 480°. This is so near the point found by my father, to wit 476°, that I shall consider his determination as correct, as he combined and compared the different ways of computation and observation, and I know the thermometer which he used to have been made with considerable care. Into a glass vessel which weighed 411 grains, I put 2236 grains of water, of the temperature of 62°. I then removed from the fire a quantity of fluid bismuth. I waited till

it

it was partially solid, at which time I inferred that both the solid and fluid metal were of the temperature of 476° . I next poured a portion of the fluid bismuth into the water. In one minute the thermometer stood at 86° ; in two minutes at $85\frac{1}{2}^{\circ}$. The true temperature at the moment of mixture was therefore $86\frac{1}{4}^{\circ}$, supposing equal temperatures to be lost in equal intervals. There was also a quantity of steam formed. I weighed the vessel with its contents, and found that it had gained 1589 grains. This therefore should be the quantity of bismuth poured in, if there had been no loss. But on drying and weighing the metal, it appeared to amount only to 1555 grains, and 34 grains consequently were lost. The bismuth was cooled $389\frac{1}{4}^{\circ}$, the water was heated $24\frac{1}{4}^{\circ}$. Then 1555 grains of water would have gained 34° . These 34° , measured by the specific heat of bismuth, as stated in Thomson's System of Chemistry at .042, are equal to $810^{\circ}.9$: But the bismuth, after becoming solid, lost $389^{\circ}.75$, which being subtracted from 810.9 , there remains $421^{\circ}.15$, which cannot be accounted for by the cooling of the solid bismuth, and must therefore be the whole or a part of the latent heat of the fused metal.

But the latent heat must be greater than this, for 411 grains of glass were also heated $24^{\circ}.25$. If the capacity of this glass be taken at .174, as Kirwan found flint glass to have, and I have found green bottle glass to have a capacity of .173 by several experiments, whence it is probable that the glass of Florence flasks has its capacity not very wide of these numbers; if then .174 be taken as the capacity of this glass, these $24^{\circ}.25$, which the bismuth communicated to the 411 grains of glass in the vessel, are, when measured by the capacity of bismuth, equal to $96^{\circ}.4$: And 1555 grains of glass would have gained $25^{\circ}.4$, which must be added to $421^{\circ}.15$ already found, and makes $446^{\circ}.55$ for the latent heat of bismuth.

But this is obviously still too little; for, as has been already mentioned, there was a good deal of steam formed. The amount of the heat thus lost is extremely difficult to assign. I shall, however, make an attempt to guess at, rather than determine it. All the 34 grains must not be reckoned to have been lost by evaporation. In spite of all my efforts, I could perceive that some, though certainly a small quantity of bismuth went off with the water, in form of a number of small particles floating in the liquid: and perhaps also a small portion

Bismuth, on the point of congealing, was poured into water, the quantities, temperatures, &c. being known; and the common temperature was taken.

Inference of the latent heat.

Correction, for the vessel;

and for steam produced.

tion of water might be wasted during the process. If we allow a half of all the loss to be accounted for in these ways, there remain sixteen grains of water which have been converted into steam. The latent heat of steam was computed, by Mr. Watt, to be equal to 940° : But this steam cannot be allowed so much heat as this; and though it may be difficult to point the quantity to be fairly granted, yet I shall expect to be within bounds when I estimate it at one half of 940° , or 470° . In this case we say, 16 grains of water have been heated 470° ; sixteen grains of bismuth would be heated by the same quantity of caloric, $11190^{\circ}.4$; and 1555 grains + 16 grains supposed to have gone off with the water, = 1571 grains of bismuth, would be heated $113^{\circ}.9$. This quantity of heat, therefore, ought to be added to the $446^{\circ}.55$ already found, and would amount, in all, to 560.45 latent heat of bismuth.

Latent heat of
bismuth deduced
 $560\frac{1}{2}$ degrees.

Other experi-
ments with bis-
muth.

I am sensible that there are several gratuitous suppositions in this last part of the reasoning, and I do not therefore lay much stress on it. I endeavoured to confirm or refute the truth of the inductions, by making an experiment exactly on the same principles, but where, by dexterity, I might prevent the formation of steam wholly or in part. In one instance I succeeded tolerably well, and then the latent heat, with every correction, amounted to nearly 600° .

I repeated these experiments for determining the latent heat of bismuth many times, and the result is expressed in the following

T A B L E:

Tabulated.

No. of Ex- periments.	Latent Heat by first Computa- tion.	Correction for the Heat re- ceived by Glass.	Whole Latent Heat.
1	457	23	480
2	411	29	440
3	412	28	440
4	465	33	498
5	480	29	509
6	438	27	465
7	465	30	495
Mean	446.8		475

On the whole, therefore, it appears that we shall not exceed the truth when we estimate the latent heat of bismuth at 550° . I made, in like manner, two experiments to ascertain the latent heat of tin, and of these the results were—

Expt. 1	520	Mean, 507° .
2	495	

Mean result
 550° for the latent heat of bismuth.
Experiments on tin.
Latent heat 507°

This agrees remarkably well with the determination of this point, said to have been made by my father, in Black's Lectures. Latent heat of zinc 493° .

Zinc is computed, by Bergman, to melt at 700° of Fahrenheit's thermometer. Taking that for granted, I made three experiments on the latent heat of zinc, in a similar way with that already related with bismuth. The results were as follow:

Experiment.	Latent Heat by first Computation.	Corrected.	Whole L. H.
1	490	28	518
2	476	22	498
3	443	32	475
Mean	469		493

In these experiments no allowance is made for loss by steam, which, however, by the dexterity acquired by practice, I was enabled to render very small. Latent heat of lead 246° .

Lead I found to melt at a point above 584° . Owing to the shortness of the thermometric scale, I could observe no higher. I suppose therefore that 594° , as found by Dr. Irvine, cannot be materially distant from the truth. Assuming it as true, I proceeded to make several experiments on the latent heat of lead. In doing this I was led immediately to notice, that melted lead does not by any means produce so much steam as other metals do when poured into water, even under the most careless management; and of this the reason will appear from the following table:

Experiments on the Latent Heat of Lead.

No.	Latent Heat.	Corrected.	Whole Latent Heat.
1	127.8	10°.3	138°.1
2	142.8	11°.	153°.8
3	149.9		
4	161.8		
5	131.5		
Mean	142.7	Mean	145°.9

In these experiments I have, by accident, lost the notes of the determination of the weight of the vessel employed in all but the two first instances: notwithstanding which it may be fairly inferred, that the mean latent heat of lead is about 150°, a quantity certainly unexpectedly small, and which, in many more experiments than these related, I was at pains to examine, without being able to discover any material inaccuracy. In the second experiment, where a little steam was formed, I ascertained the loss to be four grains. If these four grains be supposed, as in the case of bismuth, to contain 470° of heat, the computed addition to the latent heat of lead will be 20°.3, making in all a little more than 162°. This is certainly a very peculiar and unexpected quality of this metal.

Experiments on sulphur.

The only other substance which I have submitted to examination for the purpose of ascertaining its caloric of fluidity, is sulphur. The melting point of sulphur is commonly stated, in elementary works, to be at 212°: But that this is not accurate any one may convince himself, by immersing a quantity of sulphur in boiling water, where it remains altogether unaltered. By every trial which I have been able to make, I am convinced that the fusion of sulphur takes place about the temperature of 226°. I say about, because the communication of heat among the particles of sulphur is very slow, and the thermometer is often encrusted with solid sulphur, which, some how or other, certainly cools below the liquid in which it is immersed. In experiments for ascertaining this point, the thermometer ought to be kept in constant motion. The results of my experiments for finding the latent heat of sulphur, are stated in the following table.

Experiments to ascertain the Latent Heat of Sulphur.

No.	Latent Heat.	Corrected.	Whole Latent Heat.	Latent heat of sulphur $143\frac{1}{2}^{\circ}$.
1	144°.56	8°.	152°.56	
2	131°	7°.	138°.	
3	140°	4°.5	140°.5	
4	136°			
Mean	137°.89	Mean	143°.68	

In these experiments I have supposed, from experiments of my own, the capacity of sulphur to be .189, which does not materially differ from Mr. Kirwan's determination. In the other cases I have trusted chiefly to the numbers given in Thomson's Chemistry, though these are not always wholly unexceptionable. In every instance I have supposed the semi-liquid to have the temperature of the melting point, which I believe is generally true; but some practice is required to seize the moment before the frozen particles float in the fluid substance. In experiments on sulphur especially, inattention to this circumstance causes very great inaccuracy, and was the cause of considerable embarrassment to me before I observed my error.

A comparative table follows of the caloric of fluidity of all substances hitherto examined :

Substance.	Melting Point.	Latent Heat.	Ditto in Degrees measured by Capacity of Water.	Table of latent heats.
Ice	32°	155°.555°	140°	
Spermaceti	113°	145°	.	
Bees' Wax	142°	175°	.	
Tin	442°	500°	33°	
Bismuth	476°	550°	23°.65	
Lead	594°	162°	5°.604	
Zinc	700°	493°	48°.3	
Sulphur	226°	143°.68	27°.145	

In all these instances the latent heat is expressed in degrees General remarks.
measured by the capacity of the relative solid, excepting in
the cases of spermaceti and bees' wax, which are in degrees
measured by the capacity of the fluid. I endeavoured to rec-

tify this so as to make the comparison more fair, by determining the specific heat of solid wax and spermaceti; but I have not been able to satisfy myself with either of these points, owing to the softness and consequent absorption of latent heat, which a very low degree of heat induces in both these bodies. The numbers expressing their latent heats are therefore too low. On inspection of the above table, there does not appear any ratio by which the quantity of the caloric of fluidity seems to be guided: it certainly does not increase with the difficulty of fusion, but most probably has some connection with the relative capacity of each body in its solid and fluid state. The determination, however, of the capacity of any of the metals in a fluid form, excepting mercury, must be regarded as an extremely difficult task.

XI.

Strictures on Mr. DALTON'S Doctrine of Mixed Gases, and an Answer to Mr. HENRY'S Defence of the same. In a letter from Mr. JOHN GOUGH.

To Mr. NICHOLSON.

SIR,

Causes of these
strictures.

I HAVE ventured to defend the chemical union of water and air, as well as the homogeneity of the atmospherical gas. My thoughts on these subjects are briefly stated in your Journal*; and the farther prosecution of the enquiry compels me to make an open attack on my friend Mr. Dalton, and his new convert, Mr. Henry. The dispute shall be fairly conducted on my part; that is, it shall consist of arguments which I am ready to abandon as soon as they are refuted; this promise is due to friendship, as well as the obligations of truth.

Dalton's a hypothesis
not a theory.

The first thing to be ascertained is the proper appellation of Mr. Dalton's opinions. His doctrine of mixed gases is offered to the public as a mechanical theory, founded on chemical facts: a little attention, however, to Mr. Dalton's essays, will deprive it of all claim to the title of a theory. This is evident from the nature of the mechanical philosophy; every branch

of which admits of a mathematical demonstration, derived from Newton's definitions and laws of motion : but Mr. Dalton has not attempted to give stability to his new ideas, by the aid of the mathematics ; on which account the only appellation due to his doctrine, is that of a hypothesis.

No philosopher ought to disregard the means which are able to confirm his opinions, because these means may be found, upon trial, to subvert them. This I believe to be the case with my friend's hypothesis ; for I have endeavoured to shew the fallacy of it by mathematical arguments. The result of this attempt will in all probability appear in the next volume of the Manchester Memoirs ; when the merits of the essay will be determined by geometricians, who are the proper judges of such productions. This want of geometrical demonstration escapes the notice of the chemist, because my friend has seemingly supplied the deficiency by a number of probabilities of an experimental nature ; but it is almost superfluous to remind either him or your readers, that a myriad of such proofs cannot uphold a doctrine which is repugnant to the mechanical philosophy.

The two leading maxims which are derived from these probabilities, and form the basis of the hypothesis, are thus briefly expressed by Mr. Henry : " mixed gases neither attract nor repel each other, and every gas is as a vacuum to every other gas *". Mr. Dalton, reasoning from these premises, surrounds our globe with an independent atmosphere of vapour, the pressure of which preserves all the water on the earth's surface in a liquid state, and prevents the ocean itself from escaping through the air, by virtue of its own elastic force ; at least the last is a fair inference from his own conclusions.

Although I have demonstrated the existence of an atmosphere of vapour to be a mechanical impossibility, in the essay mentioned above, the reality of the thing shall be supposed at present, for the purpose of detecting the fallacy of the doctrine in the fundamental maxims of it. The truth is, Mr. Dalton has discontinued his train of reasoning too soon ; for, had his arguments been pursued to their proper limits, they would have discovered the incompatibility of the hypothesis and natural appearances. This omission is easily supplied, and will be as easily comprehended, by a person who understands the laws of hydrostatics.

A second particle may follow a former.

If a particle of vapour can pass freely through the air, a second can also succeed it at any given distance; because the latter may undoubtedly pursue the track which the former has already traversed: consequently, a series of such particles, possessing the density of water, might be raised into each perpendicular pore of the atmosphere, by the application of a proper

Air presses upon water.

force to the surface of a collection of water supporting such an atmosphere. Now the existence of such a force is certain; because when air is injected into either leg of an inverted syphon containing water, it constantly disturbs the equilibrium of this fluid; that is, the gas presses upon the liquid, notwithstanding the supposition, that the former is a vacuum to the

Air a perforated piston, by the hypothesis.

particles constituting the latter. If now we combine the postulates of the hypothesis with the preceding fact, a column of air, which occupies the upper part of a vessel containing water in its bottom, becomes a heavy piston, having its substance perforated in every direction by pores of easy transmission, which are, at the same time, separated by partitions impervious to water. After contemplating this imaginary structure of the atmospherical gas, let the reader investigate the consequences of it, and compare them with natural events. The

Air not a vacuum to vapour.

impenetrable parts of the gaseous piston would compel the water to ascend along its perpendicular pores; in which it would be kept duly condensed by the pressure of the incumbent vapour. The specific gravity of this column, compounded of air and water, would exceed that of the external air; consequently the upper extremity of it would constantly glide over the edges of the vessel into the atmosphere. Thus there would happen a double loss of water, namely, by evaporation and percolation: but nothing of the kind is observable in nature; consequently the air is impenetrable to the constituent particles of water not heated to the boiling point.

Impenetrability no obstacle to chemical union.

The circumstance of water not being able to penetrate air at low temperatures, is no obstacle to the chemical union of the two substances; for many aqueous solutions of salts occupy less space, when completed, than the materials formerly did of which they are composed. This fact has been established by the present Bishop of Llandaff; and it proves that bodies, which are mutually impenetrable, may be susceptible of the bond of chemical affinity.

One gas not a vacuum to another.

After

After all that has been said about the constitution of gases, the notion is incorrect, strictly speaking, which supposes each gas to be as a vacuum to every other. The foregoing observation may also be extended to mixtures of carbonic acid gas with water; and the following instances may be adduced in confirmation of the proposition: When a particular gas is developed in a vessel containing common air, the first portion that comes over has in it less of the specific gas than the second; nor is the second equal to the third in purity. This circumstance shews, that the new-formed gas does not find a vacuum in the air of the vessel; on the contrary, the two fluids produce a mechanical mixture, which is gradually expelled, until no part of the air remains in the tube or bottle. In like manner, if a bottle of highly aerated water be opened suddenly, the rapid expansion of the liberated gas ejects a great part of the contents, thereby proving that water opposes an obstacle to the dilatation of this gas. In reality, Mr. Dalton allows one gas to be an impediment to the motions of another; but at the same time he maintains, that two such fluids finally overcome their mutual obstructions, and occupy the same space in a state of perfect independence. This is a proposition which may be justly suspected of being a solecism in pneumatics, until the author of it has proved the contrary by a rigorous demonstration.

Proved by experiment.

Amongst the many probabilities which have been offered in support of the hypothesis, perhaps none are more ingenious than the remarks of Mr. Henry; and the part which I have taken in the present letter, obliges me to place them in a new light. This attempt must, however, be preceded by a theory, which will explain the relations of those gases that neither attract nor repel each other: such an explanation became a necessary part of pneumatics, from the time when Dr. Priestley made his experiments upon mixtures of this description. The following, then, is a sketch of a theory, having for its foundations the natural repulsion of homogeneous particles, and the reciprocal resistance which gases have been shewn to possess.

A new theory of mixed gases necessary.

When two such gases come into contact, parcels of each will be detached by every slight force, and enveloped in the substance of the other. In this manner, the two fluids will be broken to pieces, and blended in one mass forming a mechanical

Sketch of the theory.

Exception to the
general rule.

nical mixture; the component parts of which cannot be separated without the intervention of chemical agents, because the disjoined fragments of each gas will be prevented from reuniting by their mutual repulsion. There is one exception, however, to the general rule; for when a liquid is found in a mixture, the component parts will follow the law of their specific gravities; because an inelastic fluid is not actuated by an intrinsic repulsion, in all other cases the gaseous fragments will continue to be farther and farther subdivided, and will constantly assume a new arrangement from the slightest agitations. The constituent parts of such a mixture exert their force in perfect union; and this circumstance distinguishes it from Mr. Dalton's compound, the constituent gases of which press separately upon all surfaces.

The theory applied to Mr. Henry's experiments.

The necessary premises being now settled, it is time to try the powers of the theory upon Mr. Henry's experiments. If, then, ten measures of water, containing an equal bulk of carbonic acid gas, be pressed by a column of the same gas, equivalent in force to 30 inches of mercury, the state of the aqueous compound will remain invariable; because the spring of the gas in the water appears, by Mr. Henry's experiments, to be equal to the spring of the incumbent gas: therefore, should a gaseous particle happen to escape from the compound, an equal particle, from the upper part of the vessel, will replace it immediately. But if the incumbent carbonic acid be made to give place to a mass of common air of the same elastic force, the surface of the aqueous compound will undergo no change of pressure; but the gaseous part of it, meeting with no repulsion from the column of air, will begin to form a mechanical mixture with it, according to the theory. The parcels of the carbonic acid will also remain distinct, according to the same, after their escape from the water; and as a number of them will be arranged on the surface dividing the two mediums, they will form so many obstacles to the discharge of their kindred gas; the egress of which will be confined to the intermediate compartments of easier transmission consisting of common air. The division of the surface between the two mediums, into compartments of easy transmission and impenetrable points, ultimately produces a sort of equilibrium, which assigns their respective portions of the elastic acid, to the gaseous

gas compounds. The origin of this equilibrium is easily collected from the preceding theory and the laws of pneumatics. For the globules of carbonic acid, contained in the gaseous compound, invariably exert the force of 30 inches of mercury; they therefore prove too powerful for the rarefied gas of the water, which they compress, and insinuate part of their own substances into the fluid. Whilst this operation is going on at innumerable points in the surface, portions of the same gas are constantly forced through the compartments of easy transmission, by the slightest agitations. Now the quantity of gas which is discharged into the air of the jar, is greatest at first, and continually diminishes; on the contrary, the quantity that is forced into the water through the former passages, is least at first, and increases afterwards; consequently the two quantities ultimately become equal, and establish the equilibrium in question, by the contrariety of their effects.

The preceding is a general proposition, which explains a variety of appearances, such as Mr. Henry has described, by the well known principles of mechanics. Should the consideration of it be attended with conviction on his part, he perhaps will give a new turn to his experimental enquiries, and endeavour to discover the law of affinity, which connects water with the different gases. It is almost certain, that this law is not the same in all cases, as will be seen by comparing the experiments of M. la Saussure on the solution of water in common air, with Mr. Kirwan's observations on the solvent powers of hydrogen; which may be found at page 14 of the first edition of his Essay on Phlogiston.

Concluding remarks.

JOHN GOUGH.

Middlesex, Aug. 23, 1804.

P. S. The following error requires correction: In Vol. VIII. page 244, line 21, before the words, *as oft as water*, read *therefore*, and strike out the same conjunction in line 22.

XII.

An Enquiry concerning the Nature of Heat and the Modes of its Communication. By BENJAMIN COUNT of Runford, V. P. R. S. &c. Abridged from the *Philosophical Transactions for the Year 1804.*

Experiments on heat.

AFTER remarking that all discoveries on an agent of such extended operation as heat cannot fail to be eminently useful, the author proceeds to describe the apparatus used in the experiments now to be described: They were the following.

Instruments.
Thermometers.

1. Mercurial thermometers carefully constructed, having cylindrical bulbs, four inches long and four tenths of an inch in diameter, and their tubes from 15 to 16 inches long; the air being excluded, and the graduations according to Fahrenheit, exhibiting eight parts of degrees by means of a nonius.

Instruments.
Cylinders to contain hot water, and suffered to cool with different kinds of clothing.

2. Four cylindrical vessels of thin sheet brass, for ascertaining the warmth of clothing. *Fig. 1. Plate I.* The vessel is closed at both ends; but has a neck at the top, into which hot water is occasionally poured, and in which one of the thermometers is fitted and placed during the time of an experiment, so that its long bulb shall occupy the axis of the vessel, and will shew its mean temperature. Another cylindrical neck proceeds from the lower surface, and is fitted upon the adjustable part of the wooden stem beneath. The vessels are four inches long and four inches diameter, and the necks are about eight tenths of an inch in diameter, the upper one being four inches long and the lower three inches. When the vessel is clothed and charged with hot water, its rate of cooling by exposure to the quiet air of a large room, will shew the relative warmth of each particular kind of clothing.

In some of the experiments the ends of the instrument were permanently covered by the application of a thin wooden box to each, the box being varnished and covered with fine writing paper, and filled with fine eider down, and a cap of fur was pulled over the box, and the projecting neck. The clothing of these cases was applied for experiments to the cylindrical surface.

Two of the instruments (No. 1 and 2.) were thus covered up at the ends, and the other two (No. 3 and 4) were left in the state represented *Fig. 1.* without the permanent coverings.

In each experiment two similar instruments (suppose No. 1 and No. 2, or No. 3 and No. 4) were used, one *naked* and the other *covered*; so that in each experiment the naked instrument served as the standard of comparison with the other.

Method of operating with the cylinders to show the effect of clothing.

The experiments were made and registered in the following manner: the two instruments used in the experiment, placed over their wooden stands, being set down on the floor, were filled to within about $1\frac{1}{2}$ inch of the tops of their cylindrical necks with boiling hot water; and a thermometer being put into each of them, they were placed at the distance of three feet from each other, on a large table in a corner of a large quiet room, 24 feet long, 19 feet wide and 13 feet high, where they were suffered to cool undisturbed. Near them, on the same table, and at the same height above the table, there was placed another thermometer, suspended in the air to the arm of a stand, to ascertain the temperature of the air.

Every caution was used to prevent disturbance by currents or agitation of the air, whether by partial heat or the intrusion of any person during the progress of any experiment.

By the results of a great number of experiments, it was found that the same instrument cooled through any given (small) number of degrees, for instance 10° , in very nearly the same time, whatever was the temperature of the air of the room; provided always that the point from which these 10 degrees commenced, was at some constant number of degrees above the temperature of the air at the time being. The interval chosen by the Count lay between the 50th and the 40th degrees above the temperature of the air in which the instrument was exposed to cool; when for instance, the air was at 58° , the interval commenced at 108, and ended at 98° .—When the air was at $64\frac{1}{2}^{\circ}$ it commenced at 114 $\frac{1}{2}$ and ended at 104 $\frac{1}{2}^{\circ}$.

Scale through which the refrigeration was measured.

The warmth of any covering, or its power to confine heat, was estimated by the time employed in cooling through that interval.

As it sometimes happened, though very seldom, in the course of an experiment (which commonly lasted several hours) that the moment of the passage of the mercury through one or both of these extreme points was not observed, it was of importance to determine the same by interpolation from the other points observed. To do this, the author availed himself of the

Method of supplying by interpolation the extreme instants of time elapsed, when omitted to be observed.

the following law of cooling of hot bodies in a fluid, which he found by experiment to be applicable without sensible error in the present case: It is that, if the equal portions of a right line represent successive intervals of time, and perpendiculars be erected upon the same, to denote by their lengths the degrees of the excess of temperature of the hot body beyond that of the cold medium, at points denoting the corresponding instants of time, the line joining the extremities of the perpendiculars will be the logarithmic curve. Whence if two temperatures and the elapsed time be observed, it will be easy by the help of a table of logarithms to determine the time at which any intermediate temperature took place. This is exemplified by the Count, who then proceeds to relate his experiments.

Exp. 1.
A clothing of
Irish linen causes
the heat to
escape much
quicker than
from a bright
metallic surface.

Exp. 1. One of the vessels, No. 1. having its ends clothed as before described, and its polished sides naked, was filled with hot water. Another vessel, No. 2. alike in all respects, but having its sides closely clothed with Irish linen, such as is sold in London at 4s. per yard, was filled in like manner, and both were similarly exposed to cool.

The naked instrument employed 55 minutes in cooling, from 94° to 84° Fahrenheit (the air of the room being at 45°).—But the clothed instrument was cooled through the same interval in $36\frac{1}{2}$ minutes, consequently this clothing expedited the emission of heat instead of confining it.

When both instruments were cooled to 42° they were removed into a warmer room at 62° , and the clothed instrument was also found to acquire heat considerably faster than the other.

Whether the linen accelerated the cooling by assisting the succession of fresh particles of air, or by promoting the escape of heat by radiation, were points to be determined.

Exp. 2. The
same effect from
a coating of glue.

Exp. 2. To decide this, the linen of No. 2. was removed, and the sides were thinly coated with glue. In these circumstances, while the standard or naked instrument cooled through the interval in 55 minutes, the coated instrument employed only $43\frac{1}{2}$ minutes.

In reasoning upon this experiment, the author concluded, that if the glue operated only by preventing the air from attaching and fixing itself to the polished metallic surface, and consequently in that manner facilitated its circulation and the cooling; it would be of no consequence, whether the surface

was covered by one or more coatings of glue: But, on the contrary, if the radiations of heat were facilitated and increased, it might be expected that a greater effect would be produced by two coatings than by one.

Exp. 3. The experiment was therefore tried, and it was found that the instrument used employed only $37\frac{1}{2}$ minutes in passing through the interval. *Exp. 3. More coatings of glue increased the effect.*

Exp. 4, 5, 6, 7. When the experiment was repeated with clear colourless spirit varnish, the same effect was produced, and it was augmented as far as by four coatings. But on proceeding as far as eight coatings, the limit of the greatest effect was found to have been passed. *Exp. 4, 5, 6, 7. Also spirit varnish,*

Exp. 8, 9, 10. Black paint (lamp black and size) upon the varnish increased the cooling effect a little. When the instrument was cleaned and then painted, its rapidity of cooling was nearly the same = 35 minutes. And with white paint the difference was not considerable, as the effect was produced in 36 minutes. As the paint was laid on in several successive coatings, the Count remarks that little dependence is to be placed on these results as indicating a difference from colour. *Exp. 8, 9, 10. —and black size paint, —and white.*

Exp. 11. The clean instrument being smoked black over a wax candle, was found to have cooled through the interval in $36\frac{1}{2}$ minutes, while the standard employed $55\frac{1}{2}$ minutes. The lamp black, when wiped off and weighed, amounted to less than $\frac{1}{18}$ of a grain, though it had completely covered 50 square inches. *Exp. 11. And of smoke from a candle.*

With a view to a more accurate determination of the velocity of cooling, it was necessary to ascertain what heat escaped through the permanent clothing at the ends. This was done by observing the times of cooling with and without the clothing, and comparing the effects with the respective surfaces of exposure. For the whole surface of the instrument 85.195 square inches, is to the surface of its vertical sides; so is the whole quantity of heat passed off = 10000 to the quantity that passed through the same sides = 5885. And since by observation the naked instrument required $45\frac{1}{2}$ minutes to cool through the same interval as was passed through in $55\frac{1}{2}$ minutes when the ends were covered, the author concludes that $45\frac{1}{2} : 55\frac{1}{2} :: 5885 : 7015 =$ what would have passed in the latter interval through the upright surface, and consequently that the remainder of the heat = 2985 parts must have passed through the covered parts of the instrument. *Determination of the escape of heat at the flat ends of the instruments.*

By

EFFECTS OF CLOTHING, &c.

Whence the results are corrected.

By applying these results to the numbers in *Exp. 11.* where the cooling was effected in $36\frac{1}{2}$ minutes, the Count says as $55\frac{1}{2}$ minutes give 2985 heat passed through the covered ends, so will $36\frac{1}{2}$ minutes give 1942 parts. And this taken from 10000, the whole heat lost, will leave 8058 for the heat that really passed through the upright sides. But it was found that 7015 pass through the naked sides in $55\frac{1}{2}$ minutes: Whence $7015 : 55\frac{1}{2} :: 8058 : 63\frac{1}{2}$. And consequently, the corrected times are $36\frac{1}{2}$ and $63\frac{1}{2}$, which express the velocities of the passage of heat through the surface of the naked metal, and that which was blackened with smoke, viz. as 5654 to 10000 nearly.

In the same manner the velocities of the passage of heat in the experiment No. 6. are shewn to have been as 4566 to 10000.

A new course of experiments.

It has been remarked that, in these curious instances of the effects of modification of surface or clothing upon the transitions of heat, the effect may have been favoured by communication to the air, or by facilitating the process of radiation. The author's reasoning upon *Exp. 2.* appeared not so decisive as to need no support from experiments of a different class. He therefore constructed an instrument for measuring the effects of radiation, which is seen in *Fig. 2, Plate I.*

Instruments for improving radiant heat.

Like the hygrometer of Mr. Leslie,* (as the Count observes) it consists of two glass balls at the ends of a tube C and E. The tube is of such a diameter that one inch in length would contain 15 grains of mercury; the balls are 1.625 inches in diameter; the upright ends of the tube C and E are each 10 inches long; the horizontal part D is 17 inches; and the board A B, to which it is attached, is 27 inches long, 9 inches wide, and one inch thick. The pillar F supports a circular vertical screen made of pasteboard, covered with gilt paper on both sides, the use of which is to protect one of the balls from rays intended only to act upon the other. The balls contain only air, and a small drop of coloured spirit of wine is introduced by means of a short tube projecting from one of the elbows; which short tube is then hermetically sealed. By a little management the bubble of spirit is brought to rest in the middle of the horizontal tube; and when the temperature of

* Philosophical Journal, quarto series, III. 461.

the air in either of the balls is made to exceed that of the other, the increased elasticity causes the bubble to move towards the colder ball.

By a simple contrivance of sliding boards, the hot bodies were moved by rack-work and a winch to any distance from the thermoscope without the attention of the observer being taken off from the bubble in the tube, and the distances were also shewn by a graduated scale and nonius. *Fig. 3. Plate I.* shews one of these bodies. It is a metallic cylinder having its base, which is to be presented to the thermoscope, vertical, and its neck obliquely placed for the purpose of introducing the hot Water, and also a thermometer for shewing its temperature at any required time.

The experiments and observations which constitute the remainder of this memoir will be given in our next.

XIII.

Experiments and Calculations relative to physical Optics. By THOMAS YOUNG, M. D. F. R. S. From the Philosophical Transactions for 1801.

I. EXPERIMENTAL DEMONSTRATION OF THE GENERAL LAW OF THE INTERFERENCE OF LIGHT.

IN making some experiments on the fringes of colours accompanying shadows, I have found so simple and so demonstrative a proof of the general law of the interference of two portions of light, which I have already endeavoured to establish, that I think it right to lay before the Royal Society, a short statement of the facts which appear to me so decisive. The proposition on which I mean to insist at present, is simply this, that fringes of colours are produced by the interference of two portions of light; and I think it will not be denied by the most prejudiced, that the assertion is proved by the experiments I am about to relate, which may be repeated with great ease, whenever the sun shines, and without any other apparatus than is at hand to every one.

Exper. 1. I made a small hole in a window-shutter, and covered it with a piece of thick paper, which I perforated with a fine

General laws of the interference of light proved in the production of fringes of colours.

Exp. 1. Fringes bordering the shadow of a slip of card are pro-

duced by the interference of light from both edges. For interception on one side destroys the effect.

a fine needle. For greater convenience of observation, I placed a small looking glass without the window-shutter, in such a position as to reflect the sun's light, in a direction nearly horizontal, upon the opposite wall, and to cause the cone of diverging light to pass over a table, on which were several little screens of card-paper. I brought into the sun-beam a slip of card, about one-thirtieth of an inch in breadth, and observed its shadow, either on the wall, or on other cards held at different distances. Besides the fringes of colours on each side of the shadow, the shadow itself was divided by similar parallel fringes, of smaller dimensions, differing in number, according to the distance at which the shadow was observed, but leaving the middle of the shadow always white. Now these fringes were the joint effects of the portions of light passing on each side of the slip of card, and inflected, or rather diffracted, into the shadow. For, a little screen being placed a few inches from the card, so as to receive either edge of the shadow on its margin, all the fringes which had before been observed in the shadow on the wall immediately disappeared, although the light inflected on the other side was allowed to retain its course, and although this light must have undergone any modification that the proximity of the other edge of the slip of card might have been capable of occasioning. When the interposing screen was more remote from the narrow card, it was necessary to plunge it more deeply into the shadow, in order to extinguish the parallel lines; for here the light, diffracted from the edge of the object, had entered further into the shadow, in its way towards the fringes. Nor was it for want of a sufficient intensity of light, that one of the two portions was incapable of producing the fringes alone; for, when they were both uninterrupted, the lines appeared, even if the intensity was reduced to one-tenth or one-twentieth.

(To be continued.)

Experiments on heat by Count Rumford.

Fig. 1.

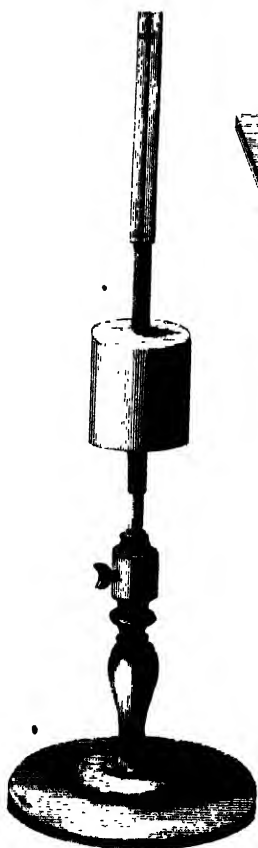


Fig. 2.

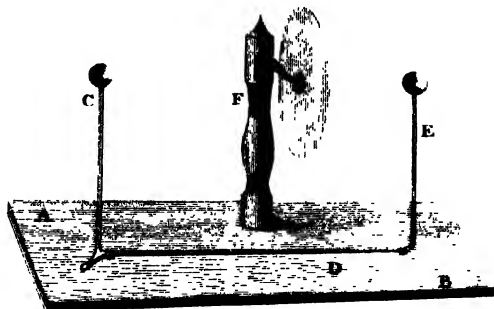
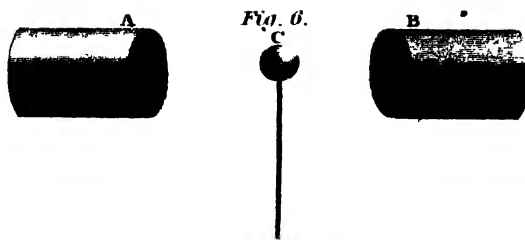
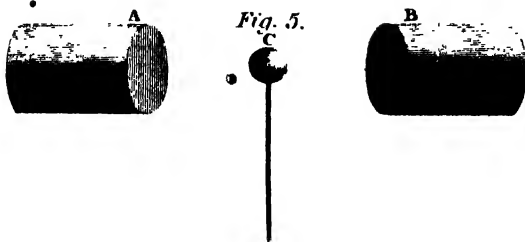
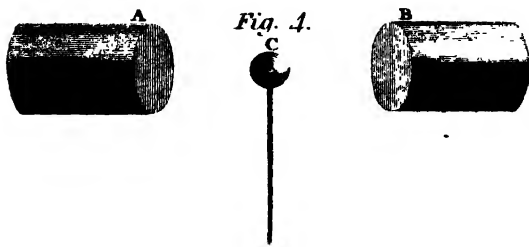
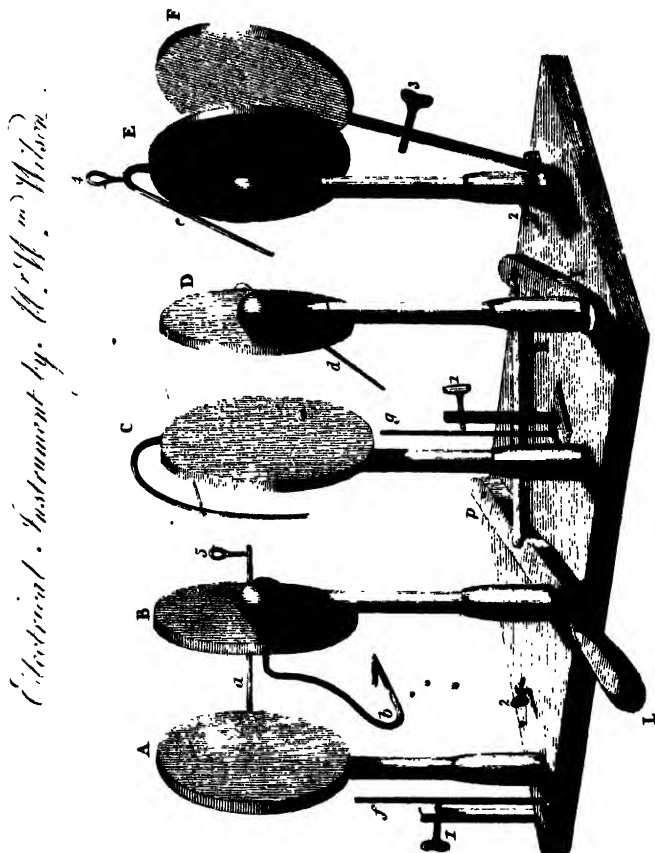


Fig. 3.

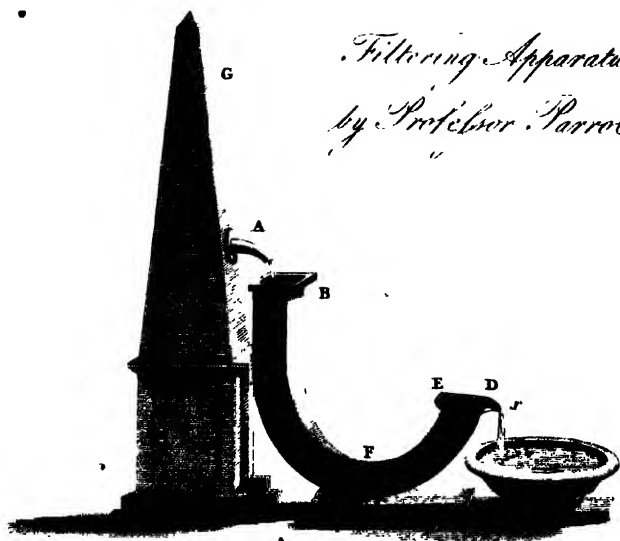


• *Experiments on heat by Count Rumford.*

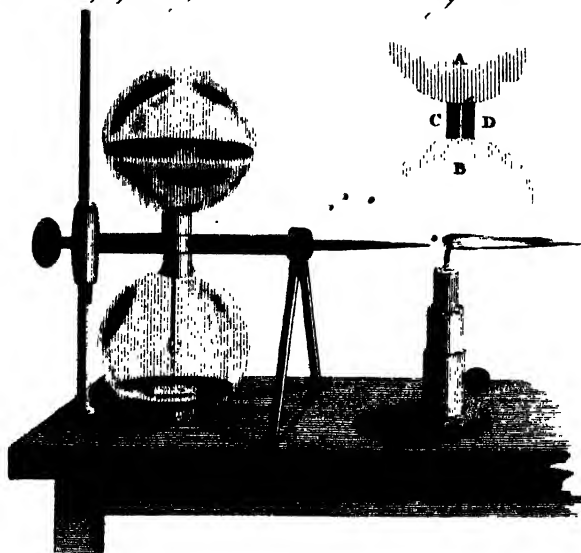




*Filtrating Apparatus,
by Professor Parrot.*



Glow-pipe by the Abbe. Melegrani.



JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

OCTOBER, 1804.

ARTICLE I.

Extract of a Letter from Count Apollon de Mousin Poushkin, to Charles Hatchett, Esq. F. R. S. describing his Method of preparing Malleable Platina. Communicated on the Request of the Count, by CHARLES HATCHETT, Esq. F. R. S. and now first published.

1. **P**RECIPITATE the platina from its solution by muriate of ammonia, and wash the precipitate with a little cold water. Purif. of platina.
2. Reduce it in a convenient crucible to the well-known spongy metallic texture, which wash two or three times with boiling water to carry off any portion of saline matter which may have escaped the action of the fire. 1. Precip. the solution by mur. of amm.
2. Reduce to the spongy metal, and wash.
3. Boil it for about half an hour in as much water mixed with one tenth part of muriatic acid as will cover the mass to the depth of about half an inch in a convenient glass vessel. This will carry off any quantity of iron that might still exist in the metal. 3. Boil in weak mur. acid, to carry off all iron.
4. Decant the acid water, and edulcorate or strongly ignite the platina. 4. Edulcorate and ignite;
5. To one part of this metal take two parts of mercury, and amalgamate in a glass or porphyry mortar. This amalgamation takes place very readily. The proper method of conducting it is to take about two drams of mercury to three drams of 5. Amalgamate with mercury:

platina, and amalgamate them together; and to this amalgam may be added alternate small quantities of platina and mercury till the whole of the two metals are combined. Several pounds may be thus amalgamated in a few hours, and in the large way a proper mill might shorten the operation.

6. Mould the amalgam into pieces; which soon become solid.

6. After the amalgam is completely produced, it must be quickly moulded in bars or plates, or any other forms that may be preferred; taking care that these moulded pieces should at least be half an inch in thickness, and of a proper length to manage them afterwards in the fire; it is also requisite that the moulds should be perfectly even and smooth. Half an hour after the pieces are formed they begin to harden by the oxidation of the mercury, and change their brilliant metallic colour for a dull leaden one.

7. Expel the mercury, by ignition:

7. As soon as the pieces have acquired a proper degree of hardness to be handled without danger of breaking, which commonly takes place in a little more than an hour, place them in a proper furnace, and keep them ignited under a muffle or in a small reverberatory. No other precaution is necessary in this operation but that of not breaking the pieces during their transport. The mercury flies off during the heat, and the platina remains perfectly solid; so that, after being strongly ignited two or three times before the bellows, it may be forged or laminated in the same manner as gold or silver; care being taken, at the commencement of the forging or of passing it between rollers, not to apply too great a force till the metal has acquired all its density. It is almost superfluous to add that in evaporating the mercury from large quantities of amalgam, a proper apparatus, such as in the silver amalgamation, must be employed to receive the volatilized mercury; but for small quantities, where the loss of this metal is of no consequence, the furnace must have a proper chimney to carry off the metallic vapours. When the platina comes out of the first fire its dimensions are about two thirteenth parts smaller every way

Lastly, Heat the platina strongly, and forge or laminate it.

To save the mercury on a large scale.

Remarks on this process.

than the original amalgam from the mould. The whole of this operation seems to be governed by the pressure of the atmosphere and the laws of cohesive attraction: for the air is driven out from between the molecules of the platina, which by their solution in mercury are most probably in their primitive and consequently uniform figure. It is very visible and at the same time a very amusing phenomenon to observe, (during the process

process of ignition, which is performed in four or five minutes) how the platina contracts every way into itself, as if pressed by some external force.*

I have also lately obtained triple salts of muriate of platina with muriate of ponderous earth; and also with muriate of magnesia; and I strongly suspect that every other earth except the siliceous, and even the metals, are susceptible of such triple combinations. I have likewise obtained a very beautiful salt of platina by the combination of soda and platina with the muriatic acid; a combination which Bergman and several other chemists deny. The best manner of obtaining it is by dissolving the platina in nitrous acid, to which, for that purpose, two parts of muriate of soda and one of platina are added. The platina must be made in a retort with its receiver, and after about four fifths of the fluid have come over, the process must be interrupted, and the whole left to cool in the sand bath. The salt crystallizes in fine prisms, which are sometimes four or five inches long, and either red brown, like titanium, yellow, like amber, or of a beautiful Coquelicot colour, according to the purity of the platina. I enclose here my address during my absence, and hope you will receive with indulgence the contents of this letter.

I am with great regard, Sir,
Your most humble and obedient servant,
COUNT APOLLOS MOUSSIN POUSSKIN.

* In the Count's letter to Mr. Hatchett, requesting him to publish the method in the text (communicated to Mr. H. some years ago) the following addition is given: (in French.)

"As soon as my amalgam of mercury is made, I compress the same in tubes of wood, by the pressure of an iron screw upon a cylinder of wood, adapted to the bore of the tube. This forces out the superabundant mercury from the amalgam, and renders it solid. After two or three hours I burn upon the coals or in a crucible lined with charcoal, the sheath in which the amalgam is contained, and urge the fire to a white heat; after which I take out the platina in a very solid state, fit to be forged."

II.

On Pepper. By THOMAS THOMSON, M. D. Communicated
by the Author.

Vegetable chemistry neglected.

NOTWITHSTANDING the great number of labourers who have engaged in the cultivation of chemistry, the field of that alluring science is too extensive to be fully occupied. While many subdivisions are left entirely waste, and others exhibit here and there only faint traces of improvement, some fortunate spots, either from their supposed importance, or from the influence of fashion or accident, have been crowded with workmen, and cultivated with enthusiastic eagerness. The mineral kingdom has probably engaged the exclusive attention of nearly two thirds of the whole body of practical chemists: The animal kingdom, in consequence of the intimate connection between chemistry and medicine, has enjoyed a considerable share of cultivation; but the vegetable kingdom, though surely not inferior in importance, and apparently rather more alluring, has till lately been greatly neglected, at least in Britain.

Reasons why this branch must be always less accurate than mineral chemistry.

Indeed it is not likely that vegetable chemistry will ever arrive at the precision which we have a right to look for in the analysis of minerals. The constituents of the latter seem scarcely susceptible of altering their state; but those of vegetables run progressively through a regular suite of changes.— Thus the substance, which in the embryo ear of corn possesses the properties of *mucilage*, appears in the ripened grain under the form of *starch*. Between these two extremes there exists an indefinite number of intermediate states, through all of which the vegetable matter successively runs. The rapidity and completion of these changes depend upon a multitude of circumstances. In no case can they proceed exactly in the same order and at the same rate unless all the circumstances tally. Without this the constituents of two vegetables even of the same species, cannot be in the same state, and of course the analysis of each, though conducted with the most perfect accuracy, will by no means exhibit an exact coincidence.

Changes in vegetable matter after life; and by reagents.

But even supposing the constituents of two vegetable bodies in every respect the same, still the analysis of each may lead

to

to different results. Vegetable substances not only pass through a suit of changes while they constitute a part of the living plant, but many of them are still susceptible of continuing the suit even after they are separated from the parent that produced them. Thus *gluten* when kept moist runs into *cheese*; *oil* when long exposed to the sun and air hardens into *wax* or *resin*, and the milky juices of plants into *gum-resins*. Our analysis frequently accelerates or occasions these changes, and even produces others altogether new. Hence the *principles* which we extract from vegetable bodies are not always the constituents of these bodies; but a new set of principles formed during the analysis, and of course varying according to the nature and circumstances of the experimental investigation. Hence we are seldom able to form again the old vegetable compound by uniting together all the ingredients which we have extracted from it.

These difficulties increase with the complicated nature of the vegetable body; for the greater the number of constituents is, the more liable are they to undergo alteration during an analysis. Indeed some vegetable principles seem incapable of existing except in combination, and are decomposed or new modified the instant we attempt to separate them. Complication of principles.

The variety of states in which the vegetable principles successively exist, together with the difficulty of examining their properties without altering their nature, has rendered it necessary for chemists to apply the names of them with greater latitude than is usual in other departments of the science. A general resemblance, indeed, in the most striking properties, seems to have been thought sufficient to entitle vegetable principles to the same name. If we examine all the bodies termed *gum*, we shall find them running on the one hand into *starch*, and on the other into *sugar*; and forming a pretty long series, having *sugar* and *starch* at the two extremities: no two of the substances constituting the series are exactly the same; but the same name is usually applied to all those that have a general resemblance. This will be easily seen by the following table, in which I have subdivided the series into five genera. Loose nomenclature of vegetables.

Genus I. Sugar - -	{	1. COMMON SUGAR	Instances in sugar, gum, starch.
		2. Sugar of grapes	
		3. Sugar of beet.	

II. Sarcocoll

- | | | |
|-----------------|---|-----------------------|
| II. Sarcocoll - | { | 1 Manna |
| | | 2. Liquorice |
| | | 3 Common sarcocoll |
| | | 4 Saccharine of malt. |
| III. Gum - - | { | 1. Mucilage of roots |
| | | 2. Cherry-tree gum |
| | | 3. GUM-ARABIC |
| | | 4. Gum of barks. |
| IV. Gluten - | { | 1. Gluten of barley |
| | | 2. Gluten of wheat |
| | | 3. Caseous principle. |
| V. Starch - | { | 1. Gum tragacanth |
| | | 2. Gum of lichens. |
| | | 3. Common starch. |

These terms are generic. The terms *gum*, *starch*, *sugar*, &c. in vegetable chemistry are not to be understood as the names of peculiar substances, nor even as *species*; but merely as *genera*, and nearly similar to the terms metals, acids, alkalies, &c. in the other departments of chemistry.

Subject introduced: *Pepper*. The preceding observations will serve, I trust, as an apology for the imperfection of the following remarks on *pepper*. I offer them to the public not as an analysis of that vegetable substance, but as an account of some of the properties of its most remarkable constituents.

Description. I. BLACK PEPPER is the fruit of an East Indian plant, the *piper nigrum*, and is too well known to require any particular description. The outer coat of the pepper-corn is brown, and a good deal shrivelled. Its taste is not nearly so pungent as the inner part; of course it contains less of the peculiar principle to which pepper owes its taste and smell.

Maceration in cold water. II. When pepper is macerated in cold water it does not lose its shrivelled appearance; a proof that the corns are impregnated with an oily substance, which prevents them from absorbing water. The liquid very soon acquires a fine deep reddish brown colour, but retains its transparency. *White pepper*, which is known to want the outer coat, communicates no such colour to water; the colouring matter then must reside in the outer coat.

The watery infusion thus obtained possesses the smell and taste which are peculiar to pepper. Like most other *extracts*, it .

it has the property of giving a red colour to vegetable blues. A very great quantity of water successively applied is necessary to exhaust the pepper of its colouring matter: but the smell and taste of pepper become less and less strong in these infusions, and at last altogether imperceptible, leaving the infusions insipid, or slightly sweetish.

1. These cold infusions contain a peculiar *extractive* matter, Peculiar *extractive* matter. which seems to reside in the outer coat of the pepper-corn. In the first infusions their matter is united to the substance, in which the taste and smell of pepper reside, and occasions its solubility in water. In the last infusions, if we judge from their appearance, it seems to be mixed with a mucelaginous substance.

2. If we mix the infusion of nut-galls with the cold infusion of pepper, no sensible change is produced; but in a decoction of pepper, it produces a copious flaky precipitate. Hence we learn that there is a substance in pepper insoluble in cold water, but separated by means of boiling water. This substance, as shall be afterwards shown, is a species of *starch*. Cold and hot aqueous infusions treated with infus. of galls.—Precip. of starch.

3. When pepper is macerated in alcohol it communicates a light yellowish green colour to the liquid, which becomes at the same time fully impregnated with the peculiar hot principle which characterizes pepper. By distilling this tincture in a retort, the alcohol is obtained colourless, but of a decidedly peppery flavour. Towards the end of the distillation, the liquid in the retort becomes muddy, and deposits a greenish matter, part of which may be observed also trickling down the sides of the receiver like drops of oil. The residual liquid is yellow, but nearly insipid. This green matter is the substance to which pepper is indebted for its taste and smell. Its properties are analogous to those of the *volatile oils*. Maceration of pepper in alcohol and distillation leave volatile oil.

4. These three bodies, namely, *extractive*, *starch*, and *oil*, I consider as the most important ingredients of the pepper-corn. Let us examine the properties of each, beginning with the oil, which is obviously the essential ingredient.

III. The colour of the oil of pepper is grass green. When first obtained it is of the consistence of turpentine, but it gradually hardens by exposure to the air. When moderately heated, it gives out a liquid oil of a yellowish green colour, and leaves a solid mass, similar nearly to a resin. When thrown into water, it sinks to the bottom of that liquid. Characters of oil of pepper.

Its

Taste and smell strong, and that of the pepper-corn.
Habitudes.
Inflammable,

Its taste is intolerably hot, and precisely similar to that of pepper. So is its smell.

When heated to 100°, it softens; it melts at 148°, evaporates a little above 212° in a white smoke, which smells like tobacco smoke, irritating the throat and exciting coughing. Evaporated to dryness on a glass plate, it leaves a yellow trace behind it. When suddenly heated, it boils violently, and the vapour burns with a clear white flame without any smoke.

Volatile by strong heat.

It gives a greasy stain of a green colour to paper. At 500° the greasy appearance is removed, but the green mark still continues, unless the heat be sufficient to char the paper. Hence I think it follows, that the colouring matter of this oil is a substance entirely distinct from the oil itself.

Soluble in alcohol and in ether, but not in water, &c.

The oil of pepper is insoluble in water; alcohol and ether dissolve it readily; the solution is light green: alcohol holding it in solution, acquires a very fragrant odour, precisely similar to that of oil of lavender. When water is added to this solution, the whole becomes milky, and passes in that state through the filter. On standing some weeks, light green flakes subside, but the milky opacity is permanent.

Action of alkalis.

Alkalies have no sensible action on this oil while cold. When thrown into liquid potash it swims on the surface. If the liquid be heated, the oil becomes brown and acquires greater consistence. At the same time it evaporates partially, diffusing around the peculiar odour of pepper.

— of nitric acid,

Nitric acid dissolves it with effervescence, the solution is yellowish brown, and a waxy matter swims on the surface. This acid acts in the same manner on other volatile oils.

— and of ox. mur. acid.

The oxygenized muriatic acid destroys the green colour and makes it yellowish white. But it still retains its former taste.

It is a vol. oil, with col. matter and resin.

These properties are sufficient to authorize us to refer this green matter to the genus of volatile oils. It is not however a pure oil, for besides the colouring matter already mentioned, it obviously contains a substance which approaches to the genus of resins in its properties.

Extractive of pepper.

IV. The **EXTRACTIVE** of pepper is procured by macerating the pepper-corns in cold water. Like the other species of this difficult genus of vegetable principles, it is scarce possible to obtain it in a state of absolute purity. But if we macerate the pepper-corns whole in successive portions of water, till the liquid loses the peppery flavour, and then pour on them fresh

water, we obtain an infusion which I believe holds in solution scarcely any thing but extractive. If this infusion be evaporated slowly in a steam heat, it leaves a brown residuum, which I consider as extractive of starch not far from purity.

1. The infusion is insipid or slightly sweetish, and of a fine reddish brown colour. As the evaporation advances, the colour deepens, and the liquid acquires an acrid flavour, similar to that of scorched vegetable matter. This flavour, which indicates a commencement of decomposition, is evolved at a very moderate temperature.

Aq. solution
by evap. leaves
starch.

2. If the evaporation be conducted rapidly the extractive is opaque, dark brown, and has a perceptible taste, and deliquesces or at least attracts moisture when first exposed to the atmosphere. But by very slow evaporation I have obtained it in fine semi-transparent brown scales, which are insipid, brittle, and not altered by exposure to the air.

3. This extractive dissolves readily in water, but not in alcohol. If it be repeatedly dissolved in water, and the solutions evaporated to dryness, a small portion of it becomes insoluble in that liquid; but the greater part continues soluble after the process has been repeated occasionally even for months.

Soluble in water,
but not in alcohol.

4. This extractive is precipitated from water by most of the metallic salts and by several of the earthy solutions.

Precipit. by
metallic salts,
&c.

It is thrown down in brown flakes by lime-water strontian water and alum. Barytes water deepens the colour, but occasions no sensible precipitate; nor is any precipitate produced by silicated potash or the magnesian salts.

It is precipitated brown red by the nitro-muriate of gold, the nitrate of silver, mercury, lead and bismuth, and by the muriate of tin and antimony. When mixed with the infusion of litmus, the colour becomes red. Ammonia restores the original colour, and at the same time throws down a copious blue lake.

5. When a current of oxymuriatic acid is passed through the infusion of pepper, the brown colour is speedily converted into a pale yellow, and the extractive precipitates in white flakes. The easiest method of making this experiment is the following: Put a quantity of the hyperoxymuriate of potash into a small retort, having a long neck (or a flask provided with a bent glass tube ground into it) and pour upon it a portion

—by ox. mur.
acid.

tion

tion of muriatic acid. Let the infusion of pepper be put into a tall glass vessel, and plunge the beak of the retort to the bottom of it. A current of oxymuriatic acid is disengaged from the salt, and passes in bubbles through the infusion for a considerable time.

When ammonia is added to the infusion thus made white, the original red colour is restored. The white flakes precipitated by oxymuriatic acid gas are insoluble in cold water.

Decoction of pepper affords with *tan*, a precip. soluble in hot, but not in cold water.

V. That pepper contains a species of starch, I conclude from the following experiment, which I have frequently repeated. When the decoction of pepper is mixed with the infusion of nut-galls, a copious precipitate falls in reddish brown flakes. If this liquid be heated to the temperature of about 120° , the precipitate is re-dissolved, but appears again when the solution cools. Now the only substance which possesses the property of forming with *tan*, a precipitate nearly insoluble in cold water, but very soluble in hot water, is starch. *Tan* indeed throws down gluten, but the precipitate is not re-dissolved in the application of heat. It throws down caoutchouc and some of the gum-resins, but the precipitate is scanty and probably owing to the extraneous matter. The precipitate which it forms with gelatine and albumen cannot even by the most careless observer, be confounded with the compound of starch and *tan*. But besides this property, which I consider as characteristic, the starch of pepper, agrees with common starch in the phenomena which it exhibits with the different chemical reagents.

This property is characteristic of starch.

Phenomena of starch with reagents.

Starch not easily decomposed.

Not soluble in alcohol, ether, or water.

Forms a jelly with hot water which is little changed.

As these phenomena have not yet been detailed by chemical writers, it may be necessary to give a short sketch of them in this place.

1. Starch is one of those vegetable bodies that are least liable to decomposition. It constitutes one of the most important articles of food, and acts an important part in the production of fermented liquors. The obvious properties of common starch are too well known to require any description.

2. Neither alcohol nor ether nor water are capable of dissolving it. The last liquid when assisted by a boiling heat, readily unites with it, and forms a kind of jelly, which may be diffused through boiling water; but when the mixture is allowed to stand a sufficient time, the starch slowly precipitates to the bottom. By drying the compound of starch and water, a brittle,

a brittle substance is obtained, differing in appearance from common starch, but exhibiting nearly the same properties with re-agents. The apparent difference is probably owing to a portion of water remaining united to the starch.

3. When starch is triturated with the hot infusion of nut-galls, a complete solution is effected. The solution is trans-^{soluble in hot}parent and rather lighter coloured than the infusion of galls. ^{infus. of galls,} When cold it becomes opaque, and a copious curdy precipi-^{and precip. by}tate falls. ^{cold.}

The infusion of nut-galls, which I am accustomed to employ in all my experiments, except when the contrary is expressly mentioned, is made by boiling together one part of galls in coarse powder and two parts of water in a glass retort. When cold, the liquid part is decanted into a glass phial. It is at first muddy, and opaque; but on standing, a sediment falls, and a transparent liquid remains of a deep brown colour, which constitutes my infusion. An ounce measure of this infusion, when evaporated to dryness in a glass vessel placed on a tin-plate box, heated by steam, leaves a brown residue, which weighs 68 grains. This residue consists chiefly of tan; for the greatest part of the extractive gradually separates from the infusion in the state of a brown, tough, imperfectly soluble membrane. Neither *extractive* nor pure *gallic acid* has any effect upon the decoction of starch. Hence the precipitate is obviously produced by the sole action of tan upon the starch. ^{The infusion of galls as prepared was tan.}

4. Twenty-four grains of starch were triturated with half an ounce measure of the infusion of galls, and mixed with about five ounces of hot water. A complete solution took place; but on cooling, the liquid became opaque, and a precipitate fell, which dried by a steam heat, weighed 35 grains. The residual liquor had a light yellow colour and an astringent taste. When evaporated to dryness, it left a residuum that weighed 17 grains. This residuum contained starch, for it was not completely soluble in alcohol. In this experiment some loss must have been sustained during the trituration. For the solid matter obtained weighed only 52 grains; or six grains less than the 24 grains of starch and the 34 grains of solid matter in the infusion of galls. The following is more to be depended on: After various trials, I found that starch and tan are capable of uniting in different proportions. But the precipitate is least soluble when $\frac{1}{4}$ oz. measure of infusion of galls is used for every 24 grains of starch.—I took 24 grains of starch, boiled them

them in a flask with five ounces of water, and then added $\frac{1}{2}$ oz. measure of the infusion of galls. On cooling, a copious precipitate fell, and the liquid remained only faintly coloured. The precipitate dried in a steam heat weighed 31 grains, and the residual liquid left 11 grains of residue. The whole amounted to 42 grains; which is very nearly equal to the 24 grains of starch and 17 of tan employed; the compound in this case consists of

58.5	starch, or nearly 3	starch
41.5	tan - - - -	2 tan
<hr/>		<hr/>
100.0		5

The compound of starch and tan is of a light brownish yellow colour, semi-transparent and brittle, and has a good deal of resemblance to common sarcocoll. Its taste is astringent; it feels glutinous between the teeth, like gum. It is very imperfectly soluble in cold water, but hot water dissolves it abundantly. Alcohol digested on it acquires a brown colour; but is incapable of separating the whole of the tan from the starch. When heated, it froths, swells and melts, and then burns with a clear flame, leaving like starch a small portion of white ashes behind it.

Infusion of starch
treated with
earths.—

5. To ascertain the effect of the earths and metallic oxides on starch, an infusion of it was formed by triturating 24 grains of starch with $4\frac{1}{2}$ ounces of water, and then boiling the mixture for some time. The decoction thus formed, is nearly transparent, and of a slight opal colour. When set aside, at least a month elapses before the starch begins to subside.

When lime water is mixed with this decoction, no change is produced; neither is any perceptible alteration occasioned by strontian water; but barytes water throws down a copious white flaky precipitate. This precipitate is re-dissolved by muriatic acid, but appears again on standing unless a considerable excess of acid has been added. Yet muriate of barytes occasions no change in the decoction of starch.

—with metallic
salts: no effect.

No metallic salt seems to have the effect of throwing down starch from its decoction. The following were tried:

Nitro-muriate of gold, platinum;
Nitrate of silver, mercury, lead;
Muriate of tin, acetite of lead;
Salts of copper, iron, zinc;
Ammoniated nickel and cobalt.

6. When

6. When potash is triturated with starch, and a small quantity of water added, the whole assumes on standing the appearance of a semi-transparent jelly. On adding water, an opal coloured solution is obtained, from which the starch is readily thrown down by an acid. When muriatic acid is employed, a peculiar aromatic odour is exhaled.

When the infusion of galls is dropt into the solution of starch in potash, a yellowish white precipitate appears, but is immediately re-dissolved, and the liquid remains opaque and of a dark brown colour. On adding muriatic acid, a copious precipitate falls, resembling the compound of starch and tan.—Nitric acid occasions no precipitate, neither does ammonia.

The decoction of starch is neither altered by potash, carbonate of potash, nor ammonia.

7. Sulphuric acid dissolves starch, and abundance of charcoal is precipitated. Diluted sulphuric acid, when assisted by heat, dissolves it without decomposition. Sulphureous acid has no effect upon it.

Diluted nitric acid first reduces starch to powder, and then dissolves it, with the exception, of some waxy matter, which swims on the surface. During the solution, some nitrous gas is exhaled.

Strong muriatic acid dissolves starch slowly, and without effervescence. When the starch does not exceed $\frac{1}{10}$ of the acid, the solution is colourless and transparent, but if we continue to add starch, a brown colour soon appears, and the acid loses a portion of its liquidity. Its peculiar smell is destroyed, and replaced by the odour which distinguishes corn mills.

Acetic acid does not dissolve starch.

8. Alcohol separates starch in part from its decoction. Potash dissolved in alcohol occasions a copious white precipitate, which is re-dissolved on adding a sufficient quantity of water. Alcohol digested on sulphuret of potash, occasions a slaky precipitate in the decoction of starch: this precipitate has sometimes an orange colour.

Such are the properties of common starch: the starch of pepper possesses them, excepting only that the colour of its precipitates is peculiarly modified by the other ingredients of pepper, from which it is not possible to free the starch completely.

A very considerable portion of the pepper-corn seems to consist of starch, chiefly united to the oil of pepper. When a

Treatment of starch with potash. Solution. Separation by acids. The infusion of starch not affected by alkalis. Starch dissolved by the ancient mineral acids. Phenomena.

Experiment with alcohol.

Starch of pepper has the preceding properties. It is united to an oil in the pepper-corn.

pepper-

pepper-corn is cut in two, we find it composed of two coats. The outermost brown shrivelled coat is easily separated by steeping the pepper in water. The second coat is much thinner and lighter coloured; it does not separate by maceration. Beneath this coat is a thick zone of pale green matter, apparently starch united to oil. This is succeeded by a thin yellow zone, seemingly of nearly the same composition. Within this is a small spherical space, sometimes hollow, but most commonly filled with a soft white substance like the pith of trees. All the zones have the peppery taste. The outer coat has the flavour, but little of the heat of pepper.

III.

Letter from the Abbé BUÉE on Mr. ROME' DE LISLE's and the Abbé HAUY's Theories of Crystallography.

(Concluded from p. 39.)

SYNTHESIS is grounded, as I mentioned, on the fact, that all well formed crystals are terminated by plain surfaces.

Primitive forms: Since there exist primitive forms, there must also be secondary forms, for the one supposes the existence of the other.

Secondary. The secondary forms are such, that sections can be made only parallel to the sides of the primitive; and when the primitive has been produced by these sections, the division being continued the integrant particles are obtained.

Laws of the structure of crystals, and theory thence resulting.

The mineralogical analysis descends from the secondary to the primitive form, and from the latter to the integrant particle; just so the mineralogical synthesis ascends from the integrant particle to the primitive, and from thence to the secondary forms. A crystalline edifice is therefore raised by means of the integrant particles. What are the laws of this extraordinary architecture? By laws I mean the disposition of the *laminæ*, not the means employed by nature to execute the curious structure.

Laws must exist, 1st, for the formation of the primitive, and 2dly, for the construction of the secondary form. The primitives are either similar to their integrant particles, or they are not. If they are not, their forms must be parallelopipedons,

pedons, and their laws of formation very simple; for there will be the same number of integrant particles in each row, as there are rows in each lamina, as there are laminæ in the primitive form. It is easy to conceive that all the joints perfectly coincide with each other, and form continued planes; neither will there be any vacuity left between the particles. If the primitive be not similar to the integrant particle, then the simplicity of the former case disappears. I have already stated that there are three forms of integrant particles; the tetraëdron, the triangular prism, and the parallelopipedon. There are also six primitive forms; the parallelopipedon, the octaëdron, the tetraëdron, the regular hexaëdral prism, the dodecaëdron bounded by rhombs all equal and similar, and the dodecaëdron with triangular sides and formed by two right pyramids united base to base. Of these six primitive forms, there are only the parallelopipedon and the regular hexaëdral prism that can exactly fill up a space without leaving any vacuity. The integrant particles of the former are parallelopipedons; of the latter, triangular prisms. As to the other four primitive forms, their integrant particles are tetraëdrons. The dodecaëdron bounded by rhombs is produced by twenty-four similar tetraëdrons without any vacuity between them; the octaëdron and tetraëdron are formed by tetraëdrons leaving octaëdral vacuities; and the dodecaëdron bounded by triangles, to be formed of tetraëdrons, must imply sections parallel to more than six planes; which perfectly coincides with observation.

These vacuities, whose existence must be admitted in the integrant particles, as well as between those particles when forming a primitive, give rise to the following reflections:

When the elements of a substance are chemically combined, that substance is homogeneous. Let us suppose a crystal of such substance to be subdivided into small parallelopipedons equal and similar; as the substance is homogeneous, and these little parallelopipedons having no vacuities between them, it is evident the elements that compose them are equal in number and proportion. We will next suppose the crystals of this substance can be divided by sections parallel to six planes. In that supposition, nineteen or twenty different species of parallelopipedons can be produced. Among these species some will be similar, others not; but none of the species will be exactly

Laws of the
fracture of
crystals, and
theory thence
resulting.

actly parallel to each other. We will proceed on two similar crystals of the same substance and equal in solidity, dividing the first into one species, the other into a different species, of parallelopipedons, equal in solidity but not in surface; and let the division of each be pushed to its last term. But as we are come by smooth sections to parallelopipedons of different species, those sections have also produced their differences: but by supposition these parallelopipedons are the result of the last possible term of division without destroying the chemical composition, and being equal in solidity, though not in surface, they cannot contain each other; therefore if their differences are not integrant parts of both, these differences must cease to be homogeneous, and we come to a sort of chemical decomposition. It is true we cannot execute this excessive division, but we can form a very correct idea of it. If the little parallelopipedons contain two sorts of elements, their differences will also, but also in different proportions; and, sir, if you will turn to Berthollet's *Researches on the Laws of Affinities*, you will see him in all his experiments proving, that however perfectly a chemical decomposition may have been made, the results will always contain a certain portion of those substances from which it was the object of the operation to separate them. If these reflections, Sir, are well grounded, do they not give us hopes, and perhaps show the possibility, of descending from the integrant particles to the constituent particles? This second research is of the same nature as the first. It is more than probable that the constituent particles themselves are divisible, having no determined figure, but are aggregations, subject to the same laws as the integrant particles. The object of the natural philosopher is not to discover the forms of the ultimate particles, but to determine their respective positions; which, if ever they could be determined in the integrant particles and their component parts, the grand problem of chemical affinities would be fully solved; and should such ever be the case, to the Abbé Haüy's theory would be due the merit. The *Encyclopædia Britannica*, under the article *Chemistry*, in the Supplement, p. 396, says:

"This theory, to say no more of it, is, in point of ingenuity, inferior to few; and the mathematical skill and industry of its author are entitled to the greatest applause.

"But

"But what we consider as the most important part of that philosopher's labours, is the method which they point out of ^{Laws of the structure of crystals, and} discovering the figure of the integrant particles of crystals; ^{theory thence resulting.} because it may pave the way for calculating the affinities of bodies, which is certainly by far the most important part of chemistry. This part of the subject, therefore, deserves to be investigated with the greatest care."

But I return to the point whence this digression carried me, to the vacuities left between the integrant particles in the construction of a primitive form. The Abbé considers them as filled either by the water of crystallization or by some other substance. It is not an admissible supposition that this other substance is composed of the same elements as the integrant particles, but in different proportions? At least, such is the conclusion I should be tempted to draw after reading Berthollet's excellent Researches on Affinities.

I shall now proceed to the laws of formation in secondary crystals. It is easy to deduce them from these two facts: viz. 1st, That the sides of the secondary crystals are planes; 2dly, That they divide by smooth sections parallel to the sides of their primitive form.

Let us take a rhomboid of carbonate of lime for example. If on one of the sides of the rhomboid I wished to raise a pyramid, I should lay laminæ of rhomboidal particles upon each other. These laminæ would decrease in surface until the last is reduced to a single rhomboid. Thus the second lamina contains fewer particles than the first, the third fewer than the second, and so on. As the faces of these pyramids are always to be planes, the successive decrements of the laminæ must be equal; that is to say, the second lamina is less by one range in every direction than the first, and the third than the second, &c. If the decrement is more rapid; that is to say, if two or three ranges are subtracted in the second lamina, the same number will be subtracted from the third, and so on successively till the pyramid is completed. As the sections are to be smooth, the joints must form one continued plane; therefore the ranges and even the particles at the joints must not encroach on each other: hence it follows that *the number of ranges successively subtracted from each lamina can never be incommensurable; that is to say, the decrement may be 1, 2, 3, 4, &c.; but never $\sqrt{2}$, $\sqrt{3}$, &c.*

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These are the decrements parallel to the edges, or, as the Abbé calls them, *decrements on the edges*. But they may take place in a parallel with the diagonal of the faces of the primitive; they are then called *decrements on the angles*, because the diagonals are drawn from one angle to the opposite angle. This second species of decrement follows the same laws as the first.

There is a third species, called by our author *intermediate decrements*. In this case they are neither parallel to the edges nor to the diagonals of the faces, but to intermediate lines, which if prolonged would intersect both the edges and diagonals, but otherwise they follow the same laws as the two first. It is a general law, therefore, *that in all cases the laminae decrease in arithmetical progression, and its ratio or the number of ranges subtracted is always commensurable*.

The particles of which the laminae are composed are to be considered as parallelopipedons; not that the integrant particles always have this figure; but if they have it not, they must leave vacuities between them, and each vacuity being added to its corresponding particle, will complete the parallelopipedon. If this was not the case, the faces of the secondary crystals would not be planes, nor could they be split smoothly in any direction. These little parallelopipedons which compose the subtracted ranges are what I called above, after our author, *subtractive particles*.

I supposed the construction of the secondary form only to take place on one of the faces of the rhomboid; but what was said relative to that face is applicable to all the others. It is also to be remarked that different laws of decrement may affect the different faces; even further, different laws may successively affect the same face. Hence a diversity of forms arise scarcely credible to a person unacquainted with the doctrine of combinations. The Abbé Haüy has calculated, "that confining one's-self to decrements by 1, 2, 3, or 4 ranges, and not taking intermediate or mixt decrements into account, the rhomboid is capable of 8,324,604 varieties of crystalline forms."

It is an important remark, that whatever may be the variety of form, the forms (in complete crystals) will always be symmetrical. There are two sorts of symmetry, the perfect and imperfect. In the perfect, the right is symmetrical with the left, and the top with the bottom; but in the imperfect, the
top

top is not symmetrical with the bottom. This latter species of symmetry appears, by general observation, to be exclusively appropriated to crystals that become electrical by heat; that is to say, which being exposed to the heat of the fire, or plunged into hot water, acquire the electric power. These crystals, the tourmaline, for example, acquire a positive electricity on one side, while on the side diametrically opposite their electricity becomes negative; and all observations hitherto made give us reason to conclude that these sides are never symmetrical, and are always produced by different or fewer laws of decrement. "Hence," says the Abbé, "by mere inspection it is easy to point out which is the side that will give the positive and which the negative electricity." (Vol. i. p. 237.)

The astonishing variety in the crystalline forms leads us naturally to ask, What can be the cause of this variety? This question has not been treated by the Abbé: allow me, Sir, to submit a few ideas on the subject for the opinion of mathematicians.

First causes, I repeat, are not the object of this discussion. I state the question thus: Why does the same subject crystallize in such a variety of forms, always symmetrical and always terminated by planes?

The solution of this question seems to require three conditions: 1st, That the particles of the substance dissolved in the fluid all leave the state of rest at the same instant, to form the crystal by their aggregation: 2dly, That, while these particles are in the act of drawing near to each other, no foreign power shall imprint on them any other motion than a common motion, whether it be in a straight line, or rotary round their common centre of gravity: 3dly, That the particles all arrive at the state of rest at the same instant, which takes place when the act of crystallization is finished. The second condition is necessary, and infers the first and third. The natural consequence of these conditions will be, that the aggregation of the particles will only take place conformably to a law acting equally on all of them, whatever may be the law.

Since they all leave the state of rest at the same instant, they are in equilibrio previous to that instant. Since they all arrive at the state of rest at the same instant, they are in equilibrio after that instant: but when particles that are acted upon by

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no other force than that which they exercise on each other, are in equilibrio, they are in the closest possible union that concomitant circumstances will permit. If the particles were in equilibrio previous to their leaving the state of rest, something must have obstructed their approach. Let us suppose that *something* to be the interposition of another substance, and that so long as the interposition remains equilibrium is maintained. But this can only be the case, in as much as the whole of the particles of the interposed substance are in equilibrio with the whole of the particles dissolved and about to leave the state of rest, which in the future I shall call the *proper particles*. If by any cause which acts uniformly on the whole surface of the dissolving fluid any of the interposed particles are subtracted, the proper particles must cease to be in equilibrio. A step toward aggregation will immediately take place, and the equilibrium will be restored. A further subtraction will produce a further step toward aggregation, and a consequent equilibrium; and these operations will be repeated so long as the cause of subtraction continues, and the longer its duration the larger will be the resulting crystalline mass. If the above mode of reasoning be admitted, it will suffice to apply the laws of equilibrium to deduce the laws of crystalline forms. The laws of equilibrium to which I allude, are those of the equilibrium of fluids, with certain modifications which shall hereafter be explained. According to these laws, that the preceding conditions may take place in the formation of a crystal, it will be necessary that they take place in the formation of each and every part of it, whatever may be the figure or the smallness of those parts. They must also take place in those last crystals which contain the least possible number of particles; and as these particles are in equilibrio, and in the greatest possible state of proximity to each other which circumstances will permit, it must follow, to fulfil all the conditions, that these particles form a symmetrical polyëdron. This peculiar disposition of the crystalline particles constitutes the modification, to which I alluded, in the laws of the equilibrium of fluids; it being necessary in this case to take the number of crystalline particles into account, which is not the case when treating of the particles of a fluid. In a fluid, the particles and their reciprocal distances are supposed infinitely small; but the crystalline particles and their distances to each other •

other must be supposed finite. This material difference will necessarily cause a difference between the forms of their aggregates. Those formed with the particles of a fluid will be bounded by curved lines; the crystalline aggregates, on the contrary, will be terminated by straight lines; and when these straight lines are not too small, the boundaries will be sensibly rectilinear. Laws of the structure of crystals, and theory thence resulting.

To ascertain what the power is that holds the particles in the state of rest, though not in close contact, is not the question; but the form of the polyhedrons which they produce. The closer adhesion of the particles to be obtained by the subtraction of caloric, sufficiently demonstrates that the particles are not in close contact with each other, and the constancy of the crystalline forms equally proves that they are in equilibrio. We will now proceed to the construction of a crystal with these crystalline particles. That the constancy of the form in the large crystal be preserved, the particles must be in equilibrio. That the equilibrium be preserved, the forces that solicit the particles to motion must mutually destroy each other. That the mutual destruction of those forces be effected, these forces after having been decomposed into other relatively parallel to three axes perpendicular to each other, and having a common point of intersection, must each meet in its direction another force equal and diametrically opposed to it. This will be obtained if the similar particles are arranged on straight lines parallel two and two at equal opposite distances from the common centre, and bisected by lines passing through that centre; but if the particles are thus arranged, they must produce symmetrical solids bounded by planes; and they are thus arranged: for if a foreign force, an excess of caloric for example, does not impede the free arrangement of the particles in the formation of the crystal, their exterior disposition will follow as much as possible their interior arrangement; but their interior arrangement must be on straight lines, or the crystal would cease to be homogeneous; their exterior disposition will therefore be on straight lines.

As the circumstances giving rise to the approach of the particles may be in the highest degree variable, it must follow that the forms produced may be diversified in the extreme. Such, Sir, is the answer I should submit for the solution of the question proposed.

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When speaking of the approach of the proper particles, I said that it might be occasioned by the subtraction of certain interposed particles which obstructed the approach of the proper particles. The former are generally water, caloric, or any fluid elastic or not. Their exit may perhaps make place for others, such as light, electricity, &c. &c. But the essential point is, that whatever these particles may be, they are in perfect equilibrio with the proper particles, otherwise they would become perturbing forces. Hence it follows, that not only the integrant particles of the crystal, but all those that are mixed with them, the chemical or component particles and even the vacuities, must follow the same laws. It also follows, that if each species of particle (even the chemical) that enters into the formation of the crystal be separately considered, each species will have its distinct symmetrical and polyædral form. The forms will penetrate each other, while the particles will not only not penetrate, but not even touch each other. All forms would stand in the same predicament as the regular octaëdron, which contains, as the Abbé Haüy has demonstrated, six regular octaëdrons and eight regular tetraëdrons, each tetraëdron containing one octaëdron and four tetraëdrons. It will further follow, if the chemical elements can be looked upon as particles which are not in contact with each other, that we may from thence mathematically determine chemical affinities.

I have now, Sir, but one task left; to speak of the application our author has made of algebra and geometry to crystallography. Many persons complain of the difficulty necessarily resulting from it in the study of mineralogy; and dare not engage in it, uncertain whether they will find a compensation for their trouble. Our author has therefore adopted a double plan, and begins by exposing his theory by a series of reasonings and arguments which will suffice to make the reader understand it, or any discoveries made in consequence of it. He then exposes the theory in the most correct of all languages—mathematical analysis; by far the most interesting, and the only means of making discoveries one's-self: and who can be callous to the pleasure of discovering an unknown truth? If the solution of a problem gives so much satisfaction, though the data be only imaginary, what must be the sensations of those who are happy enough to solve problems whose data are
set

set by *Him* whom the greatest of pagan philosophers calls the *Eternal Geometrician*? This recalls reflections to my mind which I cannot suppress. ^{Laws of the structure of crystals, and theory thence resulting.} Conversing one day with the Abbé Hally, he was taking a cursory view of all the modern discoveries; when he could not help remarking, that there was not one of them but what furnished victorious arms to the cause of religion. My answer was, that in future the name of God would be as distinctly written on a crystal as it had hitherto been in the heavens. The observation of this most religious and ingenious man reminds me of the saying of lord Bacon: "A little philosophy estranges us from religion, but a great deal reclaims us again." Even d'Alembert could not help saying, "An atheist in the Cartesian system is a philosopher mistaken in the principles; but an atheist in the Newtonian system is something worse, an insequent philosopher."

But to return to the mathematical part of our author's theory: the branch of mathematics, and the manner in which he treats it, are almost new. The theory of polyëdrons had been nearly neglected by geometers, both on account of the difficulty to represent a polyëdron on a plane, and because they did not feel the utility of the pursuit. Nevertheless, strange to say, all the regular figures that are to be found in one of the three kingdoms of nature are polyëdrons. In this point of view, the branch of mathematics illustrated by the Abbé becomes very interesting; and it is not a little so, to see with what ingenuity he extricates himself from the difficulties he meets with in his researches. He forms all the polyëdrons, however complicated, of little equal rhomboids or parallelopipeds, and by that means he reduces the theories of every possible polyëdron to that of the rhomboid, which is extremely simplified by two very simple remarks: 1st, That in all equilateral rhomboids, whatever may be the species, their projection on a plane perpendicular to their axes will always be a regular hexagon: 2dly, That the axes will always be trisected by perpendiculars drawn from all the lateral solid angles. His theory has also led him to discover in a variety of crystals geometrical properties, which must be highly gratifying to geometers. But the great advantage to be derived from it is, that it enables us with the fewest possible data to calculate the crystalline forms just as astronomers do the motions of the heavens.

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heavens. By the very means by which the latter determine the future motions of the heavens, the Abbé decides which forms are possible and which are impossible. It is thus by his simple and general law of crystallization, "the number of the ranges of the subtractive particles must always be a commensurable quantity," that he has demonstrated the regular dodecaëdron and the regular icosaëdron to be impossible forms in mineralogy. As the immortal Newton, by having discovered the law of attraction to be "in the inverse ratio of the squares of the distances," explained and calculated every thing in the vast regions of the firmament; so at the other extremity of the creation the Abbé Haüy, by means of a single law which he has discovered, explains the irregularities and calculates those problematic formations with which the mineral kingdom had hitherto astonished the natural philosopher.

Laws, Sir, that result from the study of nature, enjoy this inestimable advantage, that they always lead to *equations*; and it is only by the help of equations (expressed or understood) that questions can be solved which relate to objects that can be either counted or measured.

Of late, Sir, the word *nature* has been so much abused, that I must beg leave to state the precise sense in which I wish to be understood whenever I made use of that word in the course of this letter. The Abbé Haüy found it necessary to take a similar precaution at the beginning of the excellent work (*Traité de Physique*) he has lately published. He says: "This word *NATURE*, so frequently in our mouths, can only be looked upon as an abridged expression, either for the result of those laws which the GREAT CREATOR has imprinted on the universe, or for that aggregate of beings the works of his hands. Nature, thus viewed in its true light, is no longer a subject of cold and sterile speculation. The study of its productions, of its phænomena, ceases to be a mere exercise of the mind; it moves the heart, and strengthens the moral virtues in man, by awakening in his mind sentiments of respect and admiration at the sight of so many wonders bearing the visible characters of infinite power and wisdom."

With these sentiments I remain, Sir, yours,

A. Q. BUEE.

July 13, 1804.

IV.

Observations on Mr. GOUGH'S Strictures on the Doctrine of Mixed Gases, &c. In a Letter from Mr. J. DALTON.

TO MR. NICHOLSON.

SIR,

FROM the formidable manner in which Mr. Gough opens the campaign, I might expect him to bring a host of *facts* and *arguments* to his assistance; the *facts*, however, he prudently keeps in reserve, and it is to be feared several of the *arguments* too, as those already drawn out are scarcely of force sufficient to provoke resistance. Introduction.

Mr. Gough's first *argument* is, that the subject in dispute is more properly denominated *hypothesis* than *theory*. This is certainly not worth contending about.

Mr. Gough's next *argument* is, that the hypothesis is not mechanical; but as this cannot be made good, it seems, without a geometrical disquisition of length much exceeding an ordinary letter, the philosophical world are to wait till the same shall appear in due course. I might therefore, for the present, wave any remarks on this head, as a long and equally abstruse defence might be expected. But as I think geometry has nothing to do in the business, and that all that can be said effectually, either for or against the hypothesis, being consistent with mechanical principles, may be comprized in one short paragraph, I shall discuss the argument here. Whether the hypothesis be mechanical.

Oxygen repels oxygen, but not azote. This is a postulate. The Postulate. The
tum; and being admitted, it follows, that if a measure of parts of a gas
oxygen be put to one of azote, the oxygen finding it porous, repel each other,
must enter the pores, and *vice versa*, till the two gases severally making their way into the interstices of the other, at but not those of
last obtain a perfect equilibrium, and then press with equal other gases.
force on all the surrounding bodies, and no longer press on each other. This is so plain and obvious an inference, and so little involves any mechanical consideration, that I should have justly incurred blame for insulting my readers with the appearance of mathematical demonstration in the case. As well might I have attempted, from the elements of Euclid,
to

to demonstrate to a cottager, that if he put a sieve over his chimney the smoke would still escape, though interruptedly; or to a chemist, that if he drilled holes in an exhausted receiver, it would in time be completely filled with air.

On the action of aqueous vapour on water.

Mr. Gough next remarks upon my opinion, that the atmosphere of aqueous vapour is sufficient to prevent the ocean from escaping into the air, which he seems to think wonderful: Upon this I may remind him of another wonderful fact, which I consider the same in effect; that is, the pressure of like vapour on a cup of water in an exhausted receiver, prevents the water from escaping out of the cup. When Mr. Gough shall explain how this fact is to be accounted for, I may avail myself of his explanation to apply to the other. He proceeds, however, to demonstrate the impossibility of an aqueous atmosphere; but instead of that, he demonstrates *the impossibility of an atmosphere of any kind pressing on water, without at the same time forcing the water up into its pores*. Luckily, as Mr. Gough observes, the facts do not countenance the conclusion; and therefore, however rigid the demonstration, it shews that the previous data are not correctly assumed. The consideration of this subject is, notwithstanding, an important one; it is more than a year since I urged Mr. Gough to pay attention to it, and to attempt a solution of the difficulty which perplexes all theories of the atmosphere alike: I did it the rather, because I thought him well qualified for a subject of this nature, where the aid of mathematical science must be of subservience. The subject has never been at all explained that I know of. I have made an attempt which has not yet been published, except in a lecture last winter; but I shall still be glad to avail myself of any assistance I can obtain. Supposing that in the lowest stratum of an atmosphere incumbent on water, there is one particle of gas for one hundred of water, (which may be the case, considering their relative densities as one to a thousand); query, How does the air diffuse its pressure over all the hundred particles equally, in such sort that no column of particles of water is forced up into the interstices of the atmosphere by the inequality of the pressure? If Mr. Gough will explain this, either for his new theory or for any other, I will engage to remove all difficulty on this head which attaches to my hypothesis.

More particular statement of the fact; and question.

self of any assistance I can obtain. Supposing that in the lowest stratum of an atmosphere incumbent on water, there is one particle of gas for one hundred of water, (which may be the case, considering their relative densities as one to a thousand); query, How does the air diffuse its pressure over all the hundred particles equally, in such sort that no column of particles of water is forced up into the interstices of the atmosphere by the inequality of the pressure? If Mr. Gough will explain this, either for his new theory or for any other, I will engage to remove all difficulty on this head which attaches to my hypothesis.

Mr.

Mr. Gough next wants a rigorous proof of the proposition, Impediments afforded by one gas to the diffusion of another. that one gas affords an impediment to the motion of another, but that two such fluids finally overcome their mutual obstructions, and occupy the same space in a state of perfect independence. No one acquainted with the experimental part of pneumatic chemistry would have required proofs of such facts, because he daily experiences them. Take two phials filled with different gases; apply the mouth of one to that of the other for a few moments; upon withdrawing it, the two phials will be found to have interchanged very little of their contents. This is a most unquestionable proof that gases afford impediment to each other's motion; for, into a perfect vacuum the air rushes instantaneously. Again, let the phials remain in connection for a few minutes, or at most, hours, and they will be found to have both gases in the same proportion; and this state will continue in perpetuity afterwards. That they are ultimately independent on each other, is sufficiently marked by the circumstance, that any substance having an affinity for one of them will withdraw it from the mixture, if it will take it alone.

Mr. Gough proceeds to give a *new theory of mixed gases*: Mr. Gough's new theory of mixed gases. At the commencement of this controversy he came forth to defend the all-sufficiency of the old doctrine of the chemical union of water and air, and the homogeneity of the atmosphere, and to attack the new doctrine, which proscribes chemical union in these instances, and places for its fundamental and distinguishing maxim, that "mixed gases neither attract nor repel one another." It was therefore with no small surprise that I found, upon his advances, that a *new theory* was still requisite; but this surmise became a confirmation, when I found the preamble admitted the existence of certain, "gases that neither attract nor repel each other." The only clear information I could obtain from this sudden revolution was, that Mr. Gough uses the terms *theory* and *hypothesis* in a contrary sense to what philosophers in general do. When I brought my *hypothesis* (as he calls it) forward, it was supported by an extensive train of facts, the result of long and careful investigation, none of which, that I know of, has since been controverted; I mean what Mr. Gough alludes to as a "number of probabilities of an experimental nature:" Whereas Mr. Gough's *theory* is proposed without a single

a single fact to corroborate it, merely to try how far it may be found to agree with facts already known. Leaving Mr. Gough to develope his theory, which I confess I do not comprehend, and therefore cannot follow him in the application, I proceed to his concluding remarks on the law of affinity, which connects water with the different gases.

Water suspended
or diffused alike
in all gases.

Saussure, to whom Mr. Gough refers, in the ninth chapter of his second essay instructs us, that the *solvent powers* of common air, carbonic acid, and hydrogen, are the same as far as his experience goes. The results of Kirwan's experiments I am unacquainted with. Clement and Desorme, in the forty-second volume of the *Annales de Chimie* for 1802, have clearly shewn, that all gases take up the same quantities of water, alcohol, and ether, in like circumstances; and I think I have shewn, that these quantities are precisely the same as a torricellian vacuum of the same capacity takes up: So that unless Mr. Gough can hold out some further encouragement to the resumption of the enquiry, it seems hardly likely that any one will undertake to investigate the *diversities* in the law of affinity between gases and water, when there does not appear a single fact that points out any affinity at all in the case.

I am your's, &c.

J. DALTON.

Manchester, Sept. 8, 1804.

V.

Account of the Striking Part of an Eight-Day Clock. By Mr. JOHN PRIOR, of Nefsyfield, Yorkshire. Communicated to the Society of Arts.*

TO CHARLES TAYLOR.

SIR,

New striking
part of a clock.

I HAVE taken the liberty of sending to you a new striking-part for an eight-day clock, of my own invention and workmanship, which I finished last April; * * * * *. I beg

* Extracted from their Transactions, 1803. A reward of thirty guineas was given by the Society, who have a model of the same.

leave

leave to inform the Society of some of the advantages New striking part of a clock, arising from this new invention.

First, it consists in a wheel and fly, with six turns of a spiral line, cut upon the wheel, for the purpose of counting the hours. The pins below this spiral elevate the hammer, and those above are for the use of the detent.

This single wheel serves the purpose of count wheel, pin wheel, detent wheel, and the fly wheel, and has six revolutions in striking the twelve hours.

Permit me to suppose a train of wheels and pinions, used in other striking parts, to be made without error, and that the wheels and pinions would turn each other without shake or play. Allowing the above supposition to be true, which every mechanic knows it is not, my striking part will be found six times superior to others, in striking the hours 1, 2, 5, 7, 10, 11, and twelve times, in striking 4, 6, 8, and eighteen times, in striking 3, 9, and 12.

I have designedly made a defect in the model herewith sent, in striking 2 and 3 o'clock, to shew that what I have now advanced will, upon the trial, be found to be true.

In striking 2, I have purposely made an imperfection, equal to the space of three teeth of the wheel; and, in striking 3, an imperfection of nine or ten teeth; and yet both these hours are struck perfectly correct.

The flys in clocks turn round, at a mean, about sixty times for every knock of the hammer, but mine turns round only three times for the same purpose; and suppose the pivots were of equal diameters, the influence of oil on them would be as the number of revolutions in each. *

It would be better for clocks if they gave no warning at all, but the snail-piece to raise a weight somewhat similar to the model now sent for the inspection of your respectable Society.

Reference to Mr. PRIOR's Striking Part of his Clock.

Plate VI. Fig. 1.

A. The large wheel, on the face of which are sunk or cut the six turns of a spiral.

B. The single worm screw, which acts on the above wheel, and moves the fly C.

* See Cummings's Elements.

D.

New striking
part of a clock.

D. The spiral work of the wheel A. The black spots show the grooves into which the detents drop, on striking the hour.

E. The groove into which the locking piece F drops, when it strikes one, and from which place it proceeds to the outward parts of the spiral in the progressive hours, being thrown out by a lifting-piece H, at each hour; the upper detent G being pumped off with the locking-piece F, from the pins in the wheel A.

In striking the hour of *twelve*, the locking-piece, having arrived at the outer spiral at H, rises up an inclined plane, and drops by its own weight to the inner circle, in which the hour *one* is to be struck, and proceeds on in a progressive motion through the different hours till it comes again to *twelve*.

I. The hammer-work made in the common way, which is worked by thirteen pins, on the face of the spiral.

Fig. 2.—K. The thirteen pins on the face of the spiral, which work the hammer-work.

L. The outer pins, which lock the detent.

M. The pump-spring to the detent.

VI.

On the Cost of making Phosphorus. In a Letter from J. P.

To Mr. NICHOLSON.

SIR,

Bristol, September 16, 1804.

HAVING lately been led to consider the various processes by which phosphorus might be obtained, with a view to determine the most eligible, I thought that a very short sketch of what had occurred to me on the subject, might be not improperly introduced in a spare corner of your Journal; if you should be of that opinion, the contents of the following pages are much at your service, from a very respectful admirer of your unremitting exertions in the cause of Science and Philosophy.

J. P.

1. In

1. In the usual method of procuring phosphorus from bone-dust, and sulphuric acid, the value of the ingredients is very small, the labour of the process, with the fuel employed for the evaporation and distillation form the principal consideration in estimating the value of the product.

Estimate of
expence of ob-
taining phospho-
rus from bones.
Result 3s. per oz.

One pound of bones calcined and pulverized with half a pound of sulphuric acid, will produce, according to Fourcroy, about three quarters of an ounce of phosphorus; if the labour and fuel be estimated at two shillings, and it cannot be probably much less, the phosphorus would then cost the manufacturer about three shillings an ounce.

2. If to the acid solution obtained by the above method be added $1\frac{1}{4}$ lb. of acetite of lead, value three shillings, the phosphate of lime will be decomposed, and two ounces of phosphorus should be produced from the precipitate, (*vide Fourcroy.*) This appears then to be an improvement, since $1\frac{1}{4}$ oz. of phosphorus is procured for three shillings. But perhaps the following process may be preferable to either.

The additional
phosphorus ob-
tained by adding
acetite of lead,
is rather cheap-
er.

3. To one pound of phosphate of soda, value two shillings and six-pence, add $1\frac{1}{4}$ lb. of acetite of lead, value three shillings; above one pound of phosphate of lead will be immediately precipitated, and about one pound of acetite of soda may be easily obtained by evaporation. If this should be valued at two shillings and six-pence, the original cost of the ingredients will be reduced to three shillings, add one shilling for fuel and labour, and two ounces of phosphorus will be procured for four shillings. It is needless to remark the great superiority of the last method in point of facility, neatness and simplicity.

The process of
decomposing
phosph. of soda
by acct. of lead
affords phospho-
rus at 2s. per oz.

VII.

On the Purification of Water by Filtration; with the Description of a simple and cheap Apparatus. In a Letter from Sir HENRY C. ENGLEFIELD, Bart. F. R. S. &c.

To Mr. NICHOLSON.

SIR,

I CANNOT but think the filtering machine of Professor Parrott, published in your last Journal, a very inconvenient form of a very common instrument; but as the filtration of the

Obs. on P. Par-
rott's filtering
machine, &c.

water

water with which the metropolis is supplied contributes materially to the health and comfort of those who use it, and in fact renders it purer than almost any known spring water, a very cheap and commodious apparatus for the purpose may be considered as an object of general utility. I therefore send you a section of a machine which I constructed several years since, and which any common carpenter can make for a very few shillings, which may be thoroughly cleansed at any time, and which occupies very little room.

As you have already honoured several of my short essays, published in other collections, with insertion in your Journal, you will, I trust, not be displeased that we should become immediate correspondents.

I am, Sir,

Your obedient Servant,

H. C. ENGLEFIELD.

P. S. I will not answer for it that the machine I send you has not been already introduced into use. If it has, you will of course suppress my letter; but if it has, Professor Parrot's filtre is quite nugatory.

Arrangement
for purifying
water on a large
scale.

A most excellent arrangement for the purification of river water on a large scale is mentioned in the writings of De Luc or De Saussure, but I cannot turn to the passage in their works. It was applied with complete success by the inventor, to the stream which supplied a large town in Switzerland. The machine (if it may be so called) was as follows:

A is the upper surface of the stream to be purified, B the bottom. A cistern is sunk of six or seven feet in depth, and of a proper breadth, divided by parallel partitions, alternately rising above the surface level of the stream and open at the bottom, and level with the bed of the river, and closed at the bottom. It is obvious that the course of the water must be in the direction of the arrows, and in this repeated and slow ascent and descent, all floating impurities will be left at the top, and the heavier mixtures will subside. The cistern may be easily cleansed, either by taking out the partitions, if it is on a small scale, or by sending persons down between the walls, if it is built permanently for a great stream. Perhaps, indeed, a box having the partitions filled half the way up with sand or gravel, may on this plan be the best of all filters for domestic use.

Description

Description of the Filtering Apparatus, Plate V. Fig. 1.

A B The exterior tube, two feet high and six inches square within. Filtering apparatus described.

C D The interior tube, four inches square at the top and three inches at the bottom. It reaches within about three inches of the bottom of the exterior tube, and is covered at the bottom with a coarse linen tied round it. The use of this is to prevent the weight of the water from disturbing the sand. The upper end of this tube is formed into a funnel, for the convenience of filling it with water, and it rests on the outer tube. **E** a spout for the exit of the filtered water.

Both tubes are filled with clean washed sand up to the dotted line just below the spout. A bag for stopping the coarser impurities may be adapted to the funnel. If this machine be placed under the cock of any common water cistern, which is opened just enough to supply the funnel without running over, it will require no attendance, and will very seldom want cleaning. It is obvious that every part of the machine, when the two tubes are taken asunder is visible to the eye, and easily reached by the hand. The sand, when washed, will serve many times.

If instead of a funnel, a larger reservoir of water at the top is used, which may sometimes be convenient, it will be best to fill the upper part of the inner tube for a few inches with clean small pebbles, as the pouring in water disturbs the upper surface of the sand.

It may be made either of wood or tin, but not of lead, for fear of impregnation. It is also evident that the rapidity of action of the filtre will be in a great degree regulated by the difference of level between the spout and the surface of the water in the funnel, and by supplying the funnel with a greater or lesser stream, the machine may be made to act as quick or as slow as is wished.

As the water which supplies the metropolis is often tainted with vegetable or animal substances putrified in it, it might be well worth while to try whether filling the inner tube with powdered charcoal might not tend to free the water filtered through it from the disagreeable taste and smell communicated by the causes above-mentioned. It will also be advantageous to place the receiving vessel at some distance below the spout, that the stream may fall through as much air as it conveniently can.

Whether charcoal would be advantageous in a filtering apparatus for water.

VIII.

*Experiments on the Effects of Heat modified by Compression, by
SIR JAMES HALL, Bart. Read in the Royal Society of Edinburgh,
August 30, 1804. Communicated by the Author.*

Experiments of
heat, modified
by compression.

I BEG leave to announce to this Society the result of a series of experiments which have occupied my attention almost exclusively during several years. These experiments relate to the effects of heat modified by compression, and are intended to investigate the peculiar and characteristic principle of the Huttonian theory.

Dr. Hutton's
theory of mineral
compounds nat-
urally produced
in this way.

Dr. Hutton, in common with many former geologists, has ascribed the formation of all mineral substances chiefly to fire. But, according to him, the influence of this element has been very much modified by compression, occasioned by the weight and strength of a vast superincumbent mass, which then pressed upon what is now the surface of our globe. In this manner he has anticipated the natural objection to all igneous theories which must arise from a comparison of various mineral substances with the products of fire in our furnaces; for he conceives that pressure by repressing volatility would occasion the presence, in high temperatures, of many substances which escape in our fires, on a slight application of heat; and that these by their chemical relations would give rise to a state of things untried in any experiments hitherto published, but such as to afford a satisfactory explanation of all the natural phenomena upon his hypothesis, even of those which are the most incompatible with the common action of fire.

The facts are
that heat has so
acted in our
globe; but the
effects have not
been shewn by
experiment.

The two fundamental *postulata* required in this theory, namely, the action of heat, and the presence of a superincumbent mass are certainly allowable; since the volcanos furnish us with a proof that internal fire does act occasionally and in the irregular manner which this system requires; and since the fragmented and perturbed state of our strata enables us to say with certainty that great changes have taken place, that enormous masses have been removed, and that what was once placed at a great depth is now highly elevated.

But a third *postulatum* is involved in this theory, which seems to be of more difficult admission. Granting that heat did act
on

on substances constrained by pressure, would its action be modified? Would that modification be such as is assumed in the Huttonian theory?

To these questions Dr. Hutton has replied by arguments founded on general analogy; and has rested the proof of his hypothesis on its agreement with the phenomena of nature. In that respect few philosophical theories have been so fortunate; for its universal application to every department of the mineral kingdom, and its solution of all the difficulties, afford a concurrence of probabilities in its favour which presses on the mind with almost irresistible conviction. Still it must be owned that the basis of the system is hypothetical; and a wish has been expressed by every man of science who has attended to the subject, that this basis should be submitted to the test of experiment.

Dr. H. has endeavoured to establish these effects from geological observations.

My object has been to accomplish that end, and to bring this great question to an *experimentum crucis*. By placing substances in the predicament assigned to them in the Huttonian theory, I have endeavoured to imitate the supposed process of nature. In this attempt I have met with great and numerous difficulties, but I have at last succeeded beyond my original expectation, and have obtained results, which, if I am not greatly deceived, establish as a law of chemistry the most paradoxical of Dr. Hutton's positions.

The author recurs to actual experiment.

My experiments shew, that when pounded carbonate of lime, produced by the trituration of chalk, of marble, of the shell of a fish, or of calcareous spar, after being rammed into a small tube of porcelain, is exposed in vessels of sufficient strength and tightness to the heat of 21 or 22 of Wedgwood's pyrometer (that is to the heat in which pure silver melts,*)

Carbonate of lime agglutinates (at 22° Wedgwood) in strong close vessels, and becomes limestone.

the

* I take this opportunity of mentioning that a very material error seems to prevail with respect to this point of the pyrometrical scale. The error is the more formidable that it has been introduced and sanctioned by the highest authority possible in such a case; I mean that of Mr. Wedgwood himself. In his account of the pyrometer he gives a table, expressing the effects produced at various points of temperature, and states 28 as the melting heat of silver. Now it conflicts with my knowledge that pure silver melts at 22. I learned the fact from Dr. Kennedy, and I have had occasion to confirm the truth of it in numberless trials.

Great contrac-
tion of the
chalk.

the carbonate shrinks upon itself and agglutinates into a firm mass, which in point of hardness and specific gravity approaches very near to common lime-stone, and sometimes equals it, and which has frequently acquired the sparkling fracture, the semi-transparency, the susceptibility of polish, and the general aspect of marble. The same result is obtained when a solid piece of chalk is treated in a similar manner, and the chalk being previously measured in Wedgwood's gage, is found to contract during the action of heat three times more than the pyrometer pieces do in the same temperature. During the action of heat, the carbonate is found to have lost very little of its weight; that loss amounting in many cases to less than one per cent. and in some experiments it has undergone no sensible loss at all, or so very small a one, that it may be neglected without fear of error. When thrown into an acid, this artificial limestone effervesces violently as it dissolves, the discharge of gas continuing whilst the smallest atom of carbonate remains visible.

Imperfect fusion
of the chalk.

I have been in possession of this fact since the year 1801, and I long attempted in vain to carry the experiment farther, so as to accomplish the fusion of the carbonate. In one solitary and accidental instance, I had succeeded in obtaining it in a state of real froth, which could not have been produced without previous liquefaction; but being unable to repeat this result, I was unwilling to publish it or any of the facts already stated, till I could do so in a more satisfactory manner. In the course of last winter, with the help of many improvements in my mode of operation, and of stronger apparatus, I at last acquired the power of performing repeatedly and even with tolerable certainty, what at first had been the effect of chance.

Actual fusion.

In these experiments carbonate of lime has not only been ag-

This observation relates to the pyrometer pieces sold by the late Mr. Wedgwood, which were formed of a mixture of alumine with Cornish porcelain clay. This set having been the only one ever distributed amongst chemists, must certainly be looked upon as the standard. Other sets had previously been made by him of Cornish clay alone, which had never been sent abroad, or at least only given to some friends. It is possible that the discordance alluded to in this note, may have been occasioned by experiments made with those first sets, which may have possessed different properties from those afterwards sold.

glutinated;

glutinated, but actually fused; the substance sinking upon itself with a round and glossy surface, and exhibiting every proof of a viscid fluidity, similar to that of melting sealing wax. In general the fusion has been accompanied with a slight ebullition, which has sometimes changed the mass to a kind of froth, and sometimes has merely produced some scattered air bubbles. The whole externally and in its fracture shines much; this shine, arising in some cases from numberless facettes of crystallization, and in others from a smooth and continued gloss, like that of glass. In many specimens the crystallization of newly formed spar is distinctly visible; the crystalline mass consisting of parallel plates, which reflect together with one gloss. Some of these are discernible by the naked eye, though in general to see them well we require the help of a lens. As soon as the carbonate becomes soft, it begins to act powerfully on the tube of porcelain (generally formed of pure Cornish clay) in which it is confined; the compound shewing itself to be much more fusible than the pure carbonate. It penetrates the minutest crevices, and spreads along the cup to a considerable distance from the point of contact between the carbonate and the tube; its termination being marked by a black line, the cause of which I have not discovered. Previous to this stage of fusion no action whatever seems to take place between the carbonate and the porcelain, the former receiving from the latter an accurate impression of its shape, acquired doubtless when the powder was rammed into the tube. In this case the carbonate remains quite loose, and is often heard to rattle before the vessel is opened. Where pounded flint has been rammed into the tube in contact with the carbonate, an union has sometimes taken place, producing a substance having somewhat the appearance of chalcedony, but which shews evident proof of fusion, it having flowed so as to form little stalactites and slagmites. This substance effervesces feebly in acid, in some cases it leaves a semi-transparent cloud of undissolved matter, in others dissolves entirely the solution, yielding a jelly when evaporated to a certain pitch. This affords proof of a real union between the carbonate and the flint.

Sparry crystals
in the fused car-
bonate.

Action of the
carbonate on the
clay vessel.

Union of the
carbonate and
flint.

In all the experiments alluded to in this paper, the vessels have been exposed to a violent expansive force, by which a great number of them have been destroyed, and the experiments have often been lost or results obtained only of partial success.

When the ves-
sels have failed,
the carbonate has
shewn different
habitudes from
loss of acid.

success. But these have frequently been of value, by bringing into view important collateral facts. Thus I have found that under certain circumstances, a partial calcination has taken place by the separation of some of the carbonic acid from the lime, though enough still remained to preserve many of the leading properties of a carbonate. When a loss is sustained, amounting only to two or three, or even four per cent. I find the substance still susceptible of agglutination and fusion, but its fusibility is greatly diminished, a heat of 40 or 50 being required to accomplish what would have been done in 22 or 25, had the earth continued to be completely saturated with carbonic acid, and the carbonate thus obtained is apt to fall to decay by attracting moisture from the air. These differences afford a good illustration of the influence exerted by the acid as a flux on the earth.

—being less fusible, &c.

An apparatus resembling that of Count Rumford,

Having thus ascertained the fusibility of the carbonate under pressure of indefinite amount, I became anxious of assigning its limits, and of discovering the least force necessary for this purpose. In this view, in addition to my other devices, I followed those used by Count Rumford in trying the explosive strength of gun-powder.*

—and regulated.

By means of a great weight pressing upon a small opening, and regulated by a counterpoise adjusted at pleasure, I was able to constrain the carbonate to any given amount. In this manner I found that the pressure of 80 atmospheres, answering nearly to half a mile of sea in depth, was requisite to produce any effect of compression on the carbonate of lime, and that to execute the business well required a force four or five times greater.

Pressure 3 or 4 hundred atmospheres.

Similar experiments on bituminous matter.

I have likewise made some experiments with coal treated in the same manner as the carbonate of lime, but I have found it much less tractable, for the bitumen, when heat is applied to it, tends to escape by its simple elasticity, whereas the carbonic acid in marble is in part retained by the chemical force of quick lime. I succeeded, however in constraining the bituminous matter of the coal to a certain degree in red heats, so as to bring the substance into a complete fusion, and to retain its faculty of burning with flame. But I could not accomplish this in heats capable of agglutinating the carbonate; for I have found, where I rammed them successively into the same tube,

* *Philos. Journal*, quarto series, I. 459.

and where the vessel has withstood the expansive force, that the carbonate has been agglutinated into a good lime-stone, but that the coal has lost about half its weight, together with its power of giving flame when burnt, remaining in a very compact state with a shining fracture. Although this experiment has not afforded the desired result, it answers another purpose admirably well. It is known that where a bed of coal is crossed by a dyke of Whinstone, the coal is found in a peculiar state in the immediate neighbourhood of the Whin, the substance in such places being incapable of giving flame, it is distinguished by the name of blind coal. Dr. Hutton has explained this fact by supposing that the bituminous matter of the coal has been driven by the local heat of the Whin into places of less intensity, where it would probably be retained by distillation. Yet the whole must have been carried on under the action of a pressure capable of constraining the carbonic acid of the calcareous spar which occurs frequently in such rocks. In the last mentioned experiment, we have a perfect representation of the natural fact since the coal has lost its petroleum, whilst the chalk in contact with it has retained its carbonic acid.

Remarkable fact
of a production
resembling
blind coal.

I have made some experiments of the same kind with vegetable and animal substances. I found their volatility much greater than that of coal, and I was compelled with them to work in heats below redness; for even in the lowest red heat they were apt to destroy the apparatus. The animal substance I commonly used was horn, and the vegetable saw-dust of fir. The horn was incomparably the most fusible and volatile of the two. In a very slight heat it was converted into a yellow red substance like oil, which penetrated the clay tubes through and through. In these experiments I therefore made use of tubes of glass. It was only after a considerable portion of the substance had been separated from the mass that the remainder assumed the clear black peculiar to coal. In this way I obtained coal, both from saw-dust and from horn, which yielded a bright flame in burning.

Animal and vegetable matters
thus treated.

The mixture of the two produced a substance having exactly the smell of foot or coal tar. I am therefore strongly inclined to believe that animal substance, as well as vegetable, has contributed towards the formation of our bituminous strata. This seems to confirm an opinion advanced by Mr. Keir, which has been mentioned to me since I made this experiment. I conceive

Probability that
coal is of animal
as well as vegetable
origin.

ceive that the coal which now remains in the world is but a small portion of the organic matter originally deposited, the most volatile parts having been driven off by the action of heat before the temperature had risen high enough to bring the surrounding substance into fusion, so as to confine the elastic fluids and subject them to compression.

Horn totally volatilized under strong but not extreme pressure.

In several of these experiments, I found that when the pressure was not great, when equal, for instance, only to 80 atmospheres, that the horn employed was dissipated entirely, the glass tube which had contained it being left almost clean, yet undoubtedly if exposed to heat without compression, and protected from the contact of the atmosphere, the horn would leave a cinder or coak behind it, of matter wholly devoid of volatility. Here then it would seem as if the moderate pressure, by keeping the elements of the substance together, had promoted the general volatility, without being strong enough to resist that expansive force, and thus, that the whole had escaped. This result, which I should certainly not have foreseen in theory, may perhaps account for the absence of coal in situations where its presence might be expected on principles of general analogy.

I have shewn several specimens of these results to my friends, in particular to Lord Webb Seymour, Mr. Playfair, and Mr. Davy, who have agreed in thinking that the investigation is now brought to such a stage of advancement, that the result ought to be made public.

I propose in the course of next winter to lay before the Society a particular account of all these results, and of the methods followed in obtaining them. In the mean time I shall now submit a few of them to the inspection of the gentlemen present.

Exhibition and description of the results of carbonate exposed to heat under strong pressure.

Nos. 1, 2, 3, 4, 5, 6 and 7, were all produced in separate experiments from pounded carbonate of lime. No. 1, was amongst the first of my successful results, having been obtained in 1799. It is a firm stone, requiring a smart blow of a hammer to break it. It was inclosed in a cartridge of paper, the mark of which it still bears. The other six are still harder and more compact, approaching nearly in these qualities to common lime-stone. Nos. 2, 4, and 7, possess a degree of semi-transparency most remarkable in No. 4, and all of these specimens exhibit an uneven fracture, approaching to that of bees-

wax and marble. Their colours are variously though slightly tinged with yellow and blue; in particular No. 3, which though produced from common white chalk, resembles a yellow marble. Nos. 3, 5 and 6 have taken a tolerable polish. No. 7 contains a shell introduced along with the pounded chalk, and now closely incorporated with it.

Nos. 8, 9, 10, 11, all formed from pieces of chalk exposed unbroken to heat and pressure. No. 8 is remarkable for a shining grain and semi-transparency. Nos. 9 and 10 shew parallel planes like internal stratification which has often appeared in chalk, in consequence of the action of heat, though nothing of the kind could be seen in the native mass. No. 11, very compact, and of a yellow colour.

By various trials, to be given in detail hereafter, it appears that the carbonate in all these experiments has undergone a great diminution of bulk, amounting in some cases to more than $\frac{1}{2}$ of the original mass; and that its density has been proportionably increased. At the same time the porosity of the substance has diminished in a still higher degree. Thus it is found that chalk in its natural state absorbs and retains from 20 to 25 per cent. of water; but after being exposed to heat under compression, that it does not absorb quite 0.2 per cent. or the 500 part of its weight.

Great increase of density and closeness.

Nos. 12, 13. Examples of welding, in which the pounded chalk has been incorporated with a lump of chalk, upon which it had been rammed, so that their joining is hardly visible in the fracture.

Other specimens of bodies exposed to heat under strong pressure.

Nos. 14, 15, 16. Shewing the fusion of the carbonate well advanced, with a considerable action on the porcelain tube. In No. 15, the rod of chalk is half melted, and a yellow substance produced by a mixture of the carbonate with the porcelain. No. 16 is a lump of chalk, in a state indicating softness; a piece of porcelain, which lay in contact with it, having sunk a little into the substance of the carbonate.

Nos. 17 and 18, being delicate, are inclosed in tubes of glass. No. 17, formed from pounded chalk, shews in one part the most complete formation of spar with its rhomboidal fracture I have ever obtained. The carbonate having lost some of its carbonic acid, had crumbled so much in its essential parts by the action of the air, that the crystallization was no longer visible, and I had given up the specimen for lost till within these

Other specimens
of bodies ex-
posed to heat
under strong
pressure.

these few days. When employed in examining these results, a mass of the carbonate broke in two, and exhibited the fracture now before us nearly in as good a state as it was originally. I immediately inclosed it in a glass tube, and sealed it up with wax, so that I have hopes of preserving it. In the mean time I am happy to shew it entire to the Society. No. 18, likewise from pounded chalk is perfectly fresh and entire, though made more than a year ago; it shews some beautiful clear crystals of spar in parallel plates, but is so small as to require the use of a glass.

Nos. 19, 20, 21, shew examples of fusion and action on the tubes. In number 19, a shell is finely united to some pounded chalk. In No. 20, the mass originally of pounded chalk is sinking upon itself, and acting at the same time upon the tube. The pure carbonate in its fracture shewing brilliant facettes of crystallization. In No. 21, the carbonate in a state like the last; the compound of porcelain and carbonate shewing its liquidity by penetrating the tube so as to form a distinct vein, and then spreading on its outside to a considerable extent, terminating with the black line above alluded to.

Nos. 22, 23, 24, give proofs of entire fusion. In No. 22, we see two porcelain tubes inclosed for preservation in a glass tube, the sealed end of which must be held downwards, to shew the position in which the experiment was made. The innermost porcelain tube stands with its muzzle upwards, and the outermost covers it in the inverse position; the carbonate was contained in the inner tube. During the action of heat, the barrel failed suddenly, and the carbonate has boiled over the lips of the inner tube, running down, as here appears, almost to its bottom; thus proving that immediately previous to the failure of the apparatus, the carbonate had been in a liquid state. No. 23, two masses of carbonate, welded together in a complete state of froth. The substance shining and transparent. No. 24, two separate masses exposed together to heat; one from pounded chalk, now in a state quite like the last; the other put in as a lump of chalk dressed flat at both ends, and a letter cut on each end (as done in many of the experiments.) It is in a shining and almost transparent state; at one end the flat form and the letter are still visible; the other end is completely rounded in fusion, with a glossy surface.

Nos.

Nos. 25, 26, results of coal. No. 25, produced by the fusion of common coal under pressure in low red heat. It gave flame powerfully. No. 26, coal produced from horn. It is a shining black substance, exactly resembling pitch or petroleum, and burns with a bright flame.

JAMES HALL.

IX.

Atmospherical Air not a mechanical Mixture of the Orogenous and Azotic Gases, demonstrated from the Specific Gravities of these Fluids. In a Letter from Mr. JOHN GOUGH.

To Mr. NICHOLSON.

SIR,

I HAVE already attempted to prove common air not to be a ^{Introductory} mechanical mixture. The arguments which I used for the ^{marks.} purpose, were drawn from the properties of refracted light and the motion of sounds through elastic fluids. Such proofs are of an indirect nature; and though the mathematician may see the force of them, they may not carry an equal degree of conviction to the chemist, who ought also to be convinced. The following observations apply more immediately to the subject; and I do not perceive how they can be refuted, unless this be done by disputing the accuracy of the experiments on which they are founded. The present enquiry has the re-^{Experimental} commendation of being statical, and the data of my calculations are borrowed from certain experiments made by Mr. Kirwan and M. Lavoisier. According to the former gentleman, if the weight * of atmospherical air be denoted by 1000, that of an equal bulk of carbonic acid gas will be 1500, of oxygenous gas 1103, of azotic gas 985. An attempt to discover the comparative weights of a number of gases is a difficult undertaking; but the preceding ratios may be used with some degree of confidence, because they have been established by a philosopher of the highest reputation. Then in order to apply them to the business in hand, we will suppose the weight of 100 cubic inches of common air to be denoted by unity; though the real weight of the quantity may be stated at 31

* Essay on Phlogiston.

Equations to
find the oxygen
in air.
grains,

grains, according to Mr. Kirwan. Also let the characters w , x , and y , express respectively the cubic inches of the carbonic acid, the oxygenous, and azotic gases, which compose the mixture contained in 100 solid inches. Now $w + x + y = 100$ inches by hypothesis. But if 100 inches of air be denoted by unity, the same bulk of carbonic acid gas will be expressed by the fraction $\frac{1100}{10000}$, by the experimental data; consequently the weight of this gas in the mixture will be found by the following proportion; *i. e.* as $100 : \frac{1100}{10000} :: w : \frac{110000}{100000}$. In like manner, the weight of the oxygen is found to be $\frac{110000}{100000}$, and that of the azote is $\frac{100000}{100000}$. Now the sum of these weights is equal to unity by the premises of the calculation: if then the equation be multiplied by 100000 on the common denominator of the unknown quantities, it assumes the following form; *i. e.* $1500w + 1103x + 985y = 100000$. Thus it appears that we have only two equations, when the quantities to be determined are three; this circumstance leaves the problem unlimited; that is, the value of one of the quantities must be discovered without the aid of calculation.

Air contains carbonic acid.

M. Chaptal takes no notice of the carbonic acid in his observations on the constitution of common air; which appears to be an oversight, at least in a general view of the subject; for various processes of nature as well as art constantly discharge this gaseous acid into the atmosphere, where its presence is also indicated by quick-lime being converted into a carbonate, when placed in open situations. These facts amount to more than a probability, that the lower parts of the atmosphere contain a slight admixture of the carbonic acid; this small portion, however, was undoubtedly retained, in a great measure, by the azotic gas which Mr. Kirwan used in his experiments; because he prepared it from common air, which was confined over mercury, together with a paste of sulphur and the filings of iron.

x and y determined.

This single consideration induces me to follow the example of M. Chaptal in making w of no value; and the step may be taken with the greatest security in the present instance, because it increases the value of x , if it alter it at all; that is, the amount of the oxygenous gas, as found by calculation, will exceed the truth, on the supposition that the carbonic acid remained in the azotic gas, which Mr. Kirwan weighed. If, then, w be put equal to nothing, the preceding equations as-

sume

fume the following forms; *i. e.* $x + y = 100$, and $1103x + 983y = 100000$. Multiply the former by 985, subtract the product from the latter, and you will have the following equation; *i. e.* $118x = 1500$: Hence by division, $x = 12,4\frac{2}{3}$; and the excess of 100 above this number gives $y = 87,5\frac{1}{3}$. The steps of this process have been detailed minutely, with a view to enable the chemical reader, who has the least knowledge of algebra, to consider the grounds of the following conclusions, and to form his own judgment respecting the weight of them.

It is evident from the foregoing calculation, that if 100 parts of a mixture of the oxygenous and azotic gases, having the specific gravity of common air, were deprived of their oxygen in a graduated tube, the residuum would measure something more than 87 such parts, *i. e.* $87,5\frac{1}{3}$. But when an equal bulk of the atmospherical fluid is treated in the same manner, the portion of it which remains unabsorbed, is much less than the preceding quantity; M. Chaptal makes it to be 72 such parts, and some writers call it 78, the mean of which is 75. Now if we subtract any one of these numbers from the residuum fixed by calculation, a difference will be found, which cannot be referred to the unavoidable imperfections of eudiometers, because it could hardly escape observation in any instrument of the kind, the least excess being more than nine parts, or nearly a tenth, of the whole scale of 100 parts. On the other hand, a mixture of 72 parts azote and 28 oxygen, or of 78 of the former and 22 of the latter, exceeds an equal bulk of common air in weight; consequently the atmospherical fluid is not a mechanical mixture of the two gases in question, if any credit be due to the experimental data.

Though air has been shewn not to consist of the oxygenous and azotic gases simply mixed together, it is certainly a compound that may be resolved into these two principles: For besides supporting respiration and combustion, it converts metals into oxides, and the nitrous gas into nitric acid; therefore it contains the oxygenating principle. On the other hand, when air is employed to oxygenate bodies, the residuum of it is azotic gas of greater or less purity; consequently the atmospherical fluid is a gaseous oxide of azote, which can be decomposed by art, though chemists have not as yet discovered a certain method of producing it at pleasure, by uniting the oxygenating matter to the azotic base.

Air not a mixture of oxygenous and azotic gases.

Air a gaseous oxide of azote.

Statistical analysis
of air.

Facts have obliged me to give a name to common air, which has been hitherto exclusively applied to the dephlogisticated nitrous air of Dr. Priestly. This remark being made for the sake of perspicuity, I will endeavour, in the next place, to give a statistical analysis of the atmospherical fluid. If 100 cubic inches of common air weigh 31 grains, the weight of an equal bulk of azotic gas will be 30.535 grains; because as $1000 : 31 :: 985 : 30.535$: in like manner the weight of 100 cubic inches of oxygenous gas will be found to be 34.193 grains. The quantity of azotic gas in 100 cubic inches of air, will be stated at 75 inches in the present calculation, for the following reasons: *First*, Because 75 is the mean of 72 and 78; *second*, Because M. Lavoisier found, that four inches of oxygenous gas and 16 inches of air saturate equal quantities of the nitrous gas. These premises being settled, we shall find the weight of 75 inches of azotic gas to be 22.90125 grains; consequently the weight of the oxygen gas in 100 cubic inches of common air, is the excess of 31 above the last number, or 8.09875 grains; therefore as $34.193 : 100 :: 8.09875 : 23.685$ inches, which is the measure of the oxygen gas in 100 cubic inches of common air, when the azotic gas is stated at 75 inches. Thus it appears, that if 100 parts of the atmospherical fluid were decomposed, the elementary gases would occupy together no more than 98.685 such parts; and a difference of a like nature will be observed, if the azotic gas be called 72 or 78 *per cent.*; hence it follows that the density of air is less than that of the mechanical mixture of its elements. This position may appear paradoxical at the first view, but chemistry can furnish various instances of compounds, which are specifically lighter than the aggregates of their ingredients.

The oxygen of
the air compared
with the oxygen-
ous gas.

This analysis must remain incomplete, until the powers of the oxygen of the atmosphere have been compared with the corresponding effect of an equal weight of the oxygenous gas. Such an attempt, however, is liable to great uncertainty; because the experiments which should supply the necessary data, are variously represented by different writers. M. Lavoisier says, that four parts of oxygenous gas and nearly 16 of common air, oxygenate equal bulks of nitrous gas, namely $17\frac{1}{2}$ parts. On the contrary, M. Chaptal found by repeated experiments, that 12, or at most 13, parts of air were sufficient

cient to saturate the same quantity of nitrous gas. The data of the following calculation are taken partly from the one and partly from the other author: I have supposed with M. Lavoisier, that three inches of the oxygenous will acidify 13 inches of the nitrous gas: M. Chaptal is followed in other respects, namely, the atmosphere is imagined to contain 72 per cent. of azote, and 39 inches of air are made equivalent to 52 of nitrous gas. The calculation, which is formed upon these suppositions, will in all probability prove incorrect; but I have ventured to insert it, as being a novelty, which is likely to excite enquiry. If 39 inches of air can saturate 52 of nitrous gas, 100 inches of the former require 133.333 of the latter. Again, if 13 inches of nitrous gas demand three of oxygen, 133.333 of the former will require 30.303 of the latter. But the weight of 30.303 inches of oxygen gas amounts to 10.361 grains, which is equivalent in effect to 100 inches of air. Now 72 inches of azotic gas weigh 21.985 grains, which being taken from 31 grains, leaves 9.015 grains for the weight of the oxygenous part of 100 inches of air; in round numbers, 90 grains of the latter kind of oxygen are equal in effect to 103 of the former.

I know that M. Lavoisier, in speaking of the constitution of nitric acid, makes 100 grains of it contain 64 grains of the nitrous and 36 of the oxygenous gas; or 173 inches of the former and 105 of the latter. Had the data of the preceding calculation been taken from this ratio, the comparative superiority of atmospherical oxygen would have been much greater than it appears to be by the last paragraph. A preference, however, has been given to the preceding hypothesis, because I desire to excite enquiry, rather than expect to establish any thing of a permanent nature concerning the subject.

The preceding arguments, in conjunction with others of a kind more uncertain than themselves, suggested to me the probability of atmospherical oxygen possessing a greater degree of efficacy than an equal weight of vital air. This idea occurred to me several months ago; and I at length resolved to propose it to the public in a number of hypothetical questions, which appeared in your Journal for August. These queries are evidently borrowed from some ingenious speculations, that were published in the eighth volume of the same work*; for

The origin of the queries in the Phil. Jour. vol. viii. p. 246.

the queries imply, in common with the remarks here alluded to, that the oxygenating matter is water chemically united to the positive power of the galvanic pile. In order to accommodate this notion to the case of common air, I have supposed that the oxygen of the atmosphere receives a stronger charge of the power in question, than that which is imparted to vital air; in consequence of which it takes up an additional quantity of water, upon being disunited from the azotic basis, and is thus converted into oxygenous gas. Should future experiments discover that bodies, which are oxygenated by given portions of common air in contact with water, acquire more weight than the air loses, the discovery will undoubtedly open a wide field for enquiry.

JOHN GOUGH.

Middleham, Sept. 5, 1804.

X.

Letter from THOMAS YOUNG, M. D. F. R. S. &c. announcing the Discovery of a new moving Star, by Mr. HARDING, of Lilienthal; and on other Subjects.

To Mr. NICHOLSON.

DEAR SIR,

New planet
lately discovered.

I HAVE just received a letter from Dr. Gauss of Brunswick, F. R. S. dated September 11, in which he informs me that, a few days before, Mr. Harding at Lilienthal had discovered "a new moving star, most probably another new planet of our solar system." Dr. Gauss is certainly a person on whose judgment much dependance may be placed: He has sent some further particulars of the discovery to the Astronomer Royal.

Maximum of
density of water.

I take this opportunity of making a criticism on a work of great experimental merit which has lately appeared. It relates to the apparent expansion of water in a vessel of glass. Mr. Leslie calculates, from the supposition that the expansion of water is proportional to the square of the degrees of temperature above the freezing point, and from the expansion of glass measured by General Roy, that the apparent maximum

of density should be at 6° of the decimal scale: but he has taken in this calculation the mean rate of expansion from the freezing to the boiling point, instead of the expansion at the boiling point, which, upon the supposition, is twice as great; hence the apparent maximum, on the same grounds of calculation, should be at 3° of the decimal scale, or at 37.4° of Fahrenheit. Mr. Dalton attempts to avoid this difficulty by supposing that the thin bulb of a thermometer expands more than the glass tube employed by General Roy.

The same ingenious author has made experiments on the elasticity of different substances, by measuring the depression of the middle of a bar supported at its extremities; but if I am not much mistaken, his inferences from them are by no means accurate. Mr. Leslie gives 671625 feet for the height of a column of deal equivalent to its elasticity; the true height resulting from his experiments, taking into consideration also the inequality of curvature, appears to me to be 4664000 feet; which is still little more than half as much as would be inferred from the experiments of Chladni on the longitudinal sounds of fir wood. There must have been some inaccuracy in Mr. Leslie's experiments on steel differently tempered: for it appears from the direct experiments of Coulomb on the flexure of bars, as well as from those of Chladni, and some of my own, upon sound, that the ultimate elasticity of steel in small tensions is the same, whether it is harder or softer. This may appear at first sight paradoxical, but it admits of a sufficient explanation, which, together with many other illustrations of various parts of natural philosophy, will probably before long be laid before the public.

Your very obedient servant,

THOMAS YOUNG.

Wellbeck Street, Sep. 22, 1804.

XI.

*Description of an Instrument to ascertain the Number of Lifts made from a Mine, in any given Time. By Mr. JOHN ANTIS *.*

SIR,

Instrument for
counting the
lifts from a
mine.

I HAVE lately been encouraged by some gentlemen engaged in some coal mines in this neighbourhood, to invent a machine, which would infallibly tell the number of boxes of coals drawn out of a pit in the course of a week, or any given time. I have just now completed a model, which I think would answer the purpose extremely well.

Before, however, I make it known, I thought I would inquire of you, whether such an invention might be of a more extensive use, and as such would deserve the attention of the Society.

The machine is very simple, and need not be expensive; its properties are as follow :

1. It is of no consequence if the pit be ten, twenty, forty, or more yards deep. It will, notwithstanding this difference, only point out one box at once; and, supposing the pit to be several hundred yards deep, the principle will remain the same, and the machine could be easily adapted for it.

2. It is likewise of no consequence if the coals be drawn up by a hand windlass, or any other machine turned by horses or other powers.

3. No account is necessary to be kept for a whole week, or any given time; as the machine can be calculated for any quantity of coals whatever, that can be drawn up in a given time. Only allowance must be made for the number of persons that go down and up in the same way.

I have not yet heard of any contrivance of this kind; and therefore, if there be any such, it is unknown to me. Whoever knows the fraud, which is but too often committed by banksmen, in accounting for the quantity of coals procured from a pit, will readily admit the utility of such a contrivance. And it may most likely also be useful in tin, lead, and other

* From the Transactions of the Society of Arts, 1803.

mines.* Should the Society be of the same opinion, then I will send you a still more complete model than at present. Your speedy answer will very much oblige

*Instrument for
counting the
lifts from a
mine.*

Your most obedient Servant,

JOHN ANTIS.

Fulneck, March 6, 1802.

Charles Taylor, Esq.

SIR,

I HEREWITH send you a model of my machine, which I have before mentioned to you, for pointing out any number of boxes or baskets of coals, or other minerals, which are drawn out of a pit in any given time. I shall be much obliged to you, if you will lay it before the Society for the Encouragement of Arts, &c. for their inspection, and shall be glad that they find it of public utility.

It will easily be perceived, that by enlarging the wheels and multiplying the cogs of them, the number of yards required for the depth of a mine, as likewise the number of boxes which are wanted to be pointed out may be carried to any extent.

I am so little acquainted with coal-pits, or any other mines, that I do not know the technical terms by which the different parts are called, nor all the methods now used to draw up those materials; I must therefore beg the indulgence of the Society, if I do not always express myself with accuracy.

By inventing this machine, I aimed at ascertaining the number of boxes, my ideas respecting which I will endeavour to describe, to the best of my abilities.

First, the rope must be so long, that it can be fastened at its middle to the windlafs, and that each end thereof may reach to the bottom of the pit. This precaution will not be necessary, if it can be prevented by any other means from shifting or sliding, which I leave to others, more acquainted than I am with the present practice, to effect.

The roller A, *Plate VII. Fig. 1*, in the model, with the endless screw, represents the gudgeon in full size of a hand windlafs, such as are used in this neighbourhood, particularly where the pits are of considerable depth. As no great stress is laid upon this screw, it may be made, in order to save labour, of brass,

**Instrument for
counting the
lifts from a
mine.**

and driven, or otherwise fastened, upon any gudgeon. I suppose the windlafs to be one yard in circumference; and consequently forty revolutions of the windlafs are required before the first wheel B, upon which the brafs catch C is fixed, turns once round. The pit, therefore, may be only ten yards, or from seventy to seventy-five yards deep, yet it will make no difference. All that is required to regulate the machine to the depth of the pit, is as follows:—If the pit be little more or less than ten yards, the first wheel must be so placed in the endless screw, that the brafs catch C may be at the beginning, but just escaped from the cogs of the second wheel D. It will then only want five or six revolutions of the windlafs backwards, before the said catch is ready to operate again. But supposing the pit to be seventy yards deep, in this case, the catch C must be placed so as nearly to be ready to act. By turning backwards, the wheel may very nearly make two revolutions; and as it can be still turned as far the other way, the catch will only act once, provided the said wheel does not quite make two revolutions.

The model is about the size required for a common hand windlafs. The index will at once show how many times fifty boxes, and how many above and below that number, have been drawn up: not one of them can escape. Allowance, however, must be made for the number of persons who go up and down in the same way. Besides this, since one box is always coming up, whilst the other is going down, the numbers pointed at must be doubled, except a person was desirous to have a machine acting both ways, which would hardly be worth while. As the machine is at present, though it points out a thousand boxes only, yet it will serve for two thousand.

The same mechanism, with very little variation, may be fixed to a horse machine, called a gin, in this neighbourhood; viz. as a gin may be ten yards or more in circumference, the action would be too slow, if moved by an endless screw; a pinion, with four leaves, will be found much more convenient. This pinion is fixed to the end of the upper gudgeon, which enters the box containing the machinery. As some accuracy is required to make such a pinion work well, and these machines are often coarsely constructed, it will be necessary only to make about one inch and a half of the end of the said gudgeon exactly round, namely, that part of it which goes through the bottom

bottom of the box, and then fasten the latter so, that it can give way to any remaining inaccuracy of the other parts of the gudgeon.

Instrument for counting the lifts from a mine.

I have sent a drawing * of such a machine, which will be easily understood, since the principle is the same as in the model. But as gins are commonly made use of in deep mines, I have made it upon a larger scale; viz. the first wheel has sixty-four cogs; and this, with a pinion of four leaves, will require sixteen revolutions of the gin, for one of the wheel. Allowing ten yards for the circumference of the gin, it will make one hundred and sixty yards of rope; and considering that the said wheel can nearly make two revolutions, and that the catch will still act only once, it will consequently serve for a pit of three hundred yards deep, as well as for one of ten or twenty.

In this machine, the three wheels are placed one on another, and the index is divided; viz. the fifty on the second, and the 1000 on the third wheel. As the wheels move, the two hands are standing, which will point out the numbers as exactly as those in the model. Indeed, machines for hand windlasses may be made in the very same way, and thereby be more simple, though the difference is but small; viz. the box would require but one lid and lock, and the two smaller wheels become unnecessary.

Though in setting the machine so as to begin counting with one, it may easily be effected by lifting out the spring, and then turning the wheel with the other hand, till both hands stand upon one, yet, in the double index, the upper hand must only be set thus with the wheel, and the lower may be moved round on its socket.

In my humble opinion, my machines may be useful in two ways; first, to the honest banksman, as they will save him the trouble of noting down each box which is drawn up, and prevent mistakes; secondly, in the prevention of fraud, which will be their greatest and most important use. To obtain this end, it is necessary that the machine be so contrived, that nothing essentially wanted can be disengaged. I have therefore made the model so as to frustrate any such design. All is within the box; and neither the endless screw can be disen-

* This drawing is in the Society's possession.

Instrument for
counting the
lifts from a
mine.

gaged from the wheel, nor the spring; nor can the number on the index be altered without first opening the box. This latter can be made of iron, if found necessary, with a good lock to it; otherwise, the stress is so very little, that even one of wood, made in all respects like the model, would answer, if it could be secured from the effects of the weather. In the machine likewise, represented by the drawing, nothing can be altered, except the box be first opened. This machine is fastened to the beam in which the upper gudgeon turns. To effect this, the bottom of the box must have two projecting ends with holes in them, larger than necessary to receive a strong pin or rivet; which pins must have large heads, so that when properly driven or riveted in, the box may be able to give way to any remaining inaccuracy in the lower parts of the gudgeon, as I before observed. Those machines for hand windlasses, like the model, are fixed to the side of the post into which one of the gudgeons turns; but perhaps these might likewise be contrived so, that the gudgeon with the endless screw may be a piece by itself, attached only to the windlass by a square, or otherwise, as is often practised in other machineries, such as cotton-mills, &c.

I am, Sir,

Your obedient Servant,

JOHN ANTIS.

Fulneck, May 18, 1802.

Charles Taylor, Esq.

Reference to the Model sent by Mr. ANTIS to the Society.

Plate VII. Fig. 1. A. The roller, with an endless screw, to be the size of the gudgeon of a hand windlass.

B. The first wheel, of forty teeth, with a brass catch C. It may be moved backwards or forwards by the endless screw.

D. A ratchet wheel with fifty teeth, which, when the wheel B is put in motion one way, the catch will slide over the teeth of it; but, on turning it the other way, the catch will drop into and forward this wheel one tooth, in which situation it is prevented from returning by the spring E, placed on the side of the box which holds a tooth on the side of the wheel.

F. A small pinion of four teeth, on the back part of the axle of the wheel D. This pinion moves a large wheel on the other side of the box.

Fig. 2. G. The large wheel last mentioned : it has eighty teeth. Instrument for
counting the
lifts from a

H. A smaller wheel of fifty teeth, fixed fast on the face of the wheel G ; which wheel H works a wheel I, of the same number of teeth, shown by dotted lines behind the figured dial-plate.

K. The short hand, or finger of the dial, is placed upon an arbor or collar of this wheel I, and moves with it.

The long finger, or hand of the dial-plate, goes upon a square on the end of the spindle on which the above arbor moves ; which spindle moves forward this long finger one division in fifty of the outer circle marked on the dial-plate, every time the rope, or any thing attached to it, has been down to the bottom of the mine, and returned to the top.

The inner circle of the face of the dial is marked in divisions of fifty each. When therefore fifty draughts up and down have been made from the mine, the shorter hand will be found, if both hands were originally set correctly at the top, to have moved to the first interior division marked 50 ; and so on, in proportion, will advance as more draughts are made.

K, Is a board placed under the wheels B D, in *Fig. 1*, and which separates them from the other wheel-work in *Fig. 2*, where only a few teeth of the wheel B appear behind it.

L L, Show the temporary handles in both figures ; and the position of those letters denote that the gudgeon of the windlafs, when the machine is in actual use, should be there joined to, or make a part of the roller A.

Fig. 3, Shows, on an enlarged scale, the form of the catch C. The steel spring M pressing against the pin N, returns the catch to the tooth of the wheel D, when it has been forced back ; and a pin fixed underneath the catch moves in a groove O, made in the wheel B, to prevent the catch being pressed too far back, or thrown out improperly by the spring.

A door is fixed on each side the box, and should be locked, to prevent the hands being altered unknowingly.

The drawing of a machine for similar purposes, intended to be used with a gin or horse-wheel, alluded to in Mr. Antis's letter, is in the Society's possession, if a reference thereto be thought necessary.

XII.

On Galvanism. In a Letter from RA. THICKNESSE, Esq.

To Mr. NICHOLSON.

SIR,

Wigan, Sept. 20, 1804.

On the causes
which retard
discovery.

ALTHOUGH the production of the electric fluid by the galvanic pile has never yet been satisfactorily accounted for, it appears to me somewhat easy to be explained, from a consideration of the principles of chemistry. This, I am aware, is a very bold opinion from a man who is a mere dabbler in philosophy; but as it must be allowed that the ablest philosophical inquirers and experimentalists have been guilty of extraordinary oversights, I trust my presumption is excusable. When an ardent mind has once entered a wrong path in pursuit of knowledge, it is too intent upon the objects before it to turn aside, and too anxious to proceed to be induced to look back. Thus chemists fifty years ago thought they obtained earth from water, because they omitted to weigh the vessels in which they made their experiments; and thus, perhaps, the professors of galvanism at the present day, being of opinion, as I believe they all are, that the electric fluid proceeds from the metals, devote little of their attention to the fluid employed in the pile, considering it almost as a mere conductor. Nothing can be more obvious to a chemist now than the necessity of weighing the matter he subjects to experiment, and also the product; yet the discovery alone of this necessity overturned the opinions, and falsified the wisdom, of all the previous cultivators of the science, and led to the modern theory of chemistry and its improvements; an omission how trifling in itself to keep men (philosophers too) poking in the dark for ages!

Argument or
inference that
water is com-
posed of oxygen,
hydrogen, and
electricity.

Water, we are told, is composed of certain parts of oxygen and hydrogen; but, as to form these bodies, from a state of gas at least, into water, it is necessary to pass an electric spark through, or rather *into* them (no matter whether the process of formation be combustion or not), and as water owes its fluidity to heat, the matter of heat and the electric fluid being probably the same, or modifications of the same body, I think it reasonable to assume that water is composed of oxygen, hydrogen, and the electric fluid.

The

The experiments by which M. Volta and others have endeavoured to shew that piles composed of metals only afford the galvanic fluid, are so little demonstrative of it, that they seem to me equally illustrative of the contrary; and I believe that all galvanic piles must, with the intervention of fluids, be composed of two metals, or *other substances*, which have an affinity, the one for oxygen and the other for hydrogen; for water and other fluids made use of in galvanic piles, are decomposed; and not, I conceive, as hath generally been imagined, by the electricity of the metals; but by the action of affinities. For instance, in a pile formed with zinc, copper, and water, the oxygen and hydrogen of the water having a stronger affinity for the zinc and copper (the oxygen for the zinc and the hydrogen for the copper) than for each other, unite with them, the water being consequently decomposed, and the electric or galvanic fluid, which was contained in it, consequently set at liberty.

In support of this theory I may alledge as facts, that the first shock from a galvanic pile is generally the strongest, when the energy of the affinities is the greatest; that for a repetition some interval of time is requisite, whilst the decomposition is taking place; that the metals in the pile act, not according to their quantities, but according to their surfaces—the greater these are, the greater being the quantity of water acted upon; that the zinc is always oxidated, and the copper (or silver) always acted upon by the hydrogen, being rendered more brittle, &c.; that any alteration in the arrangement of the pile, which brings two pieces of the same metal to the same stratum of water, instead of one piece of each metal (one to act on the oxygen and the other on the hydrogen), interrupts the process, and, no decomposition taking place, there is no electric fluid produced; and it may also be added, that this last is a fact which cannot otherwise be accounted for.

I am not ignorant that a stream of the galvanic fluid from the pile, passed through water, decomposes it; but it is no proof, I presume, that water contains none of this fluid, because it is decomposed by a greater quantity than will chemically mix with it. If we pour a small quantity of water impregnated with carbonic acid gas into lime-water, the lime is precipitated, or *composed*, the lime-water becoming turbid; but

Decomposition of fluids in galvanism ascribed, not to the action of electricity, but to chemical affinity.

Fuller explanation from the facts.

Objection, that the galvanic stream decomposes water, answered.

but if we add a further quantity, the lime is again decomposed, and the lime-water becomes again perfectly clear.

I am, Sir,

Your most obedient servant,

RA. THICKNESSE.

XIII.

Description of an Instrument for drawing in true Perspective from Nature, and of another of considerable Simplicity and Cheapness for delineating Ovals. In a Letter from a Correspondent, R. B.

To Mr. NICHOLSON.

SIR,

Introduction.

AS I observe that you are willing, in your capacity of Journalist, to lay before the public any sketch or outline of invention that may promise to be useful, whether in its ultimate state of improvement or not, I am encouraged now and then to send my thoughts, queries, observations, or news, as they may occur. The following instruments are offered to your notice, in hopes they may appear in your excellent collection.

Instrument for drawing in perspective.

Fig. 1, Plate VIII. is a sketch of an instrument for perspective, made some years ago by Dolland, and of which I know not the inventor. A telescope or camera is suspended vertically on a frame by an universal joint or jimbals. Horizontal rays A, are directed down the tube by a plane mirror B, and are again rendered horizontal, and turned to the eye through a side hole in the tube, by another mirror C. At the lower end is a pencil E sliding in a well-fitted socket, and pressed gently downwards by a weight or spring; or still better, by the hand only. The result or use is, that while the images are in succession brought into apparent contact with a point in the field of view, the pencil may be employed in tracing them in true perspective upon the table beneath*.

* There is an omission of the grey or rough glass, if the drawing be meant for a camera; or of the eye-piece, if it be a telescope. The first focal convergence must be made in these, and not at the eye.—N.

Fig. 2 represents a simple rule and string for drawing ovals ^{Instrument for drawing ovals.} on paper. A C B is a filken thread, fixed at A, and capable of being lengthened, shortened, and fixed by a screw B at the other end. This screw B can be placed, by a longitudinal groove in the ruler, at any distance from A, and can be made to pinch the thread upon any one of the divisions of the rule. At C is a pencil to be moved in the bend of the thread. It must be held upright, and it would be easy to contrive means of keeping it so; but it does not seem an object of sufficient necessity to add to the price of the instrument.

In the use, set A at one focus of the intended oval and B at the other. Allow the string to extend till the pencil marks the extremity of the conjugate diameter. Draw the semi-oval by moving the pencil along in the stretched thread: Then reverse the points A and B, placing them respectively on the foci occupied before by each other. Draw the other semi-oval, which completes the figure.

I am, Sir,

Your obliged correspondent,

R. B.

XIV.

On the Computation of Tables of Squares and Cubes. In a Letter from H. G.

To Mr. NICHOLSON.

SIR,

Sept. 8, 1804.

AS I can employ my time in a more useful, pleasant, and advantageous way than by making tables of any kind, I have no desire on my own account to trouble your readers with the following remarks, and shall therefore leave it entirely to your choice to notice them or not in your Journal. ^{Introduction.}

I am, Sir,

Your humble servant,

H. G.

YOUR correspondent E. O. in his note, p. 79 of your Journal for this month, has very properly pointed out an error ^{On the computation of tables of squares and in cubes.}

On the computation of tables of squares and cubes.

in the rule laid down by me in p. 150 of your Journal for July last, which will be corrected by substituting the word *given* for the word *next*.

But he does not seem to be aware that the rule I proposed for finding the squares of roots in an arithmetical progression of those roots, is precisely the same as that he has used and elucidated in pages 5 and 6 of your Journal for this month.

Neither has he made me a convert to his doctrine, that addition should be used in preference to subtraction; for the one appears to me as easy an operation as the other, for all the purposes to which it is applied in the construction of the tables in question.

With respect to the construction of the table of cubes, I will barely state my method of computing the cubes of the same roots E. O. has calculated, in pages 8th and 9 of your Journal for this month, and will leave it to those who may be inclinable to enter on the investigation, to determine which method will be most advantageously practised, his or mine, only observing, that E. O. has a continual repetition of the first differences, which I have entirely avoided without adding any figures, in other respects, to those he has used.

I still think my method of computing the table of cubes is not more liable to inaccuracy than that E. O. has adopted; but in order to avoid almost the possibility of error in my computation, and at the same time to add to, examine, and correct Mr. Councers table, I would place the cubes of half the given roots $\times 8$. at such distances under each other, as would enable me to interpolate my calculations of the cubes of twice those roots, as it is done in the left hand part of the following elucidation; and then it is evident, every other cube obtained by the calculation must be the same as that obtained from the table; and thus a proof of the accuracy of Mr. Councers table, and of the continuation of it, would go hand in hand.

H. G.'s Method to obtain the Cubes of certain Roots.

Cubes. Roots.		T A B L E,		Cubes of H. G.'s Roots	
		Supposed Mr. Cooner's.		per contra, or	
		Roots.	Cubes.	Cubes of twice the Roots opposite.	
$a = 26560$	$= 18736316416000 = A$	- - - - -	$13280^3 = 2342039552000 \times 8$	$\times 8 =$	18736316416000
$b = 26559$	$= 18734200194879 = B$				
		$2116221121 = C = A - B$			
		$159360 = D = 6a$			
$c = 26561$	$= 18738432796481 = E = D + C + A$				
		$2116380481 = F = E - A$			
		$159366 = G =$			
$d = 26562$	$= 18740549336328 = H = G + F + E$		$13281^3 = 2342568667041 \times 8$	$\times 8 =$	18740549336328
		$2116539847 = I = H - E$			
		$159372 = K = G + 6 = 6d$			
$e = 26563$	$= 18742666035547 = L = K + I + H$				
		$2116699219 = M = L - H$			
		$159378 = N = K + 6 = 6e$			
$f = 26564$	$= 18744782894144 = O = N + M + L$		$13282^3 = 2343097861768 \times 8$	$\times 8 =$	18744782894144
		$2116858597 = P = O - L$			
		$159381 = Q = N + 6 = 6f$			
$g = 26565$	$= 18746899912125 = R = Q + P + O$				

COMPUTATION OF SQUARES AND CUBES.

Letter

XV.

Letter to the Editor from Mr. WILLIAM HENRY, in reply to Mr. Gough.

Manchester, September 13, 1804.

SIR,

Previous remarks.

NOTHING was farther from my intention, when I communicated to you the "Illustrations of Mr. Dalton's Theory of Mixed Gases," than to enter into controversy respecting a doctrine, to the defence of which I may naturally be supposed to be much less competent than its author. Yet it is certainly required of me, both by the respectful attention due to your correspondent Mr. Gough, and to others of your readers, either to explain and support, or to relinquish, if erroneous, the opinion respecting which I have publicly expressed a coincidence with Mr. Dalton. The last alternative I do not, at present, feel disposed to adopt, because I am far from being convinced by Mr. G.'s reasoning, and in the explanation, which I am about to offer, I shall confine myself to those proofs of the new theory of mixed gases, which are furnished by my own experiments; leaving to Mr. Dalton the more important office of establishing its fundamental evidences.

The quantity of gas absorbed being as the pressure, the last is taken to be the cause,

It is by no means clear to me, whether or not Mr. Gough denies the principle, "that the relation between gases and water is altogether a mechanical one."* To me this appears as legitimate an inference, as can possibly be deduced in physics; for the quantity of every gas, absorbed by water, follows exactly the ratio of the pressure: And, since it is a rule in philosophizing, that effects of the same kind, though differing in degree, are produced by the same cause, it is perfectly safe to conclude, that every, even the minutest portion of any gas, in a state of absorption by water, is retained entirely by incumbent pressure. There is no occasion, therefore, to call in the aid of the law of chemical affinity, when a mechanical law fully and satisfactorily explains the appearances. And when the effect ceases, it is equally conformable to just reasoning to infer, that this happens, in every case, solely in consequence

* Certain acid gases, the muriatic for instance, are obviously excluded.

of the relaxation or removal of that mechanical power, which held the gas in its situation. Under all circumstances, therefore, when a gas escapes from water, whether by placing the aqueous mixture under an exhausted receiver, or in an atmosphere of a different gas, the cause operating its escape, must be one and identical, viz. the diminution of mechanical pressure. Before we account for any effect by comparison of affinities, the affinities themselves should be proved to exist. But with respect to the relation of gases to water, the proof fails in every instance; for how can that effect be fairly ascribed to chemical affinity which is destroyed, as is the connection of every gas with water, by an unmixed mechanical cause; and is it not absurd to compare powers which have no existence in nature?

Admitting then the connection between gases and water to be entirely dependent on physical pressure, there naturally arises out of this law, an explanation of the curious fact, which I have ascertained, that each gas, when absorbed by water, is retained in its place by an atmosphere of no other gas but of its own kind. Under any other atmosphere, the absorbed gas escapes, even without agitation, though this certainly accelerates the event. Now the subaqueous gas can only accomplish its change of place by virtue of some active principle or power inherent in it, and causing its movement; and this power is its elasticity, which is not counteracted by that of an incumbent gas of a different sort. The fact affords, therefore, something further than "probability," that the particles of gases press only on those of their own kind; for to say that the elasticity of the subaqueous gas is not counteracted by any incumbent one of a different sort, is to assert in other terms, that the one is not pressed by the other,—the principle which I am solicitous to establish. —and the pressure is of no other gas than that absorbed.

The above remarks are the only ones which I deem it within my province to urge in reply to Mr. Gough. Before closing this letter, however, I must exculpate myself from the charge of attempting to uphold a mechanical theory, by probabilities drawn from chemical facts. This statement is not correct; the facts which I have alledged are purely statical, and my object has been to prove that they were before erroneously included under the laws of chemical affinity, with which, in reality they have no connection. These mechanical phenomena, I have

The theory is an induction from facts; capable of mathematical treatment, but founded on observation.

have brought in support of the general principle "that the particles of gases press only on those of their own kind;" and in so applying them, I trust I have conformed to the rigorous laws of philosophical induction. Such general principles are not uncommon in natural science; and though, like the law of gravitation and some other less comprehensive ones, they may be mathematically pursued and investigated, yet they are not derived from mathematical reasoning, but from a method of enquiry which Newton himself did not disdain to employ,—that of induction on the basis of experiment and observation. The speculations of Mr. Dalton, being founded on well ascertained facts, appear therefore to me to be fairly entitled to the appellation of *theory*, and not to be included within the definition of *hypothesis*,* handed down to us from the father of experimental philosophy. I remain, very truly, Sir,

Your's, &c.

WILLIAM HENRY.

XVI.

Description of a very simple and cheap Contrivance for making Port-Folios of large Dimensions. By the late JAMES MALTON, Esq.†

SIR,

Method of constructing large port-folios.

AS I well know the great inconvenience experienced by artists and collectors of prints and drawings, from the want of portfolios of dimensions capable of inclosing large subjects, and as I also well know that the means used by the Society for the Encouragement of Arts, &c. to promulgate knowledge and useful information are earnest as they are extensive, I am induced to lay before that body a port-folio of my construction, which I persuade myself possesses every advantage that can be wished.

The difficulty, or rather the impossibility of obtaining cases or port-folios, as large as are sometimes requisite, has given rise to many expensive contrivances, to the same end; or large

* "Quicquid ex phaenomenis non deducitur, *hypothesis* vocanda est." Princ. LIII. in Bruckeri Hist. Crit. Phil. Tom. IV. p. 646.

† Society of Arts, 1803.

prints, &c. must be kept in rolls, to their almost certain destruction, by frequency of rolling; or at least they are thus exposed to the danger of being crushed by accident. Milled pasteboards, of which port-folios are made, are not manufactured above a certain moderate size: to exceed that size in a port-folio, is an undertaking of no inconsiderable trouble, in pasting, glueing, and pressing them together. On inquiring of Mr. Newman, of Soho-square, (a manufacturer of these articles) how he managed to make port-folios above the ordinary dimensions, he informed me, it was an undertaking of trouble, and related his having made one for a gentleman, by attaching sixteen of the largest milled boards together; that the materials alone cost five guineas; and that its weight was greater than one man could lift.

Method of constructing large port-folios.

My method of construction obviates all disadvantages—weight, expence, and trouble; and port folios of any dimensions may very readily be manufactured by the simple application of two straining-frames, covered on both sides with canvas, and papered; and connected, as all port-folios are, by leather at the back, or with wooden backs, the sides being connected by hinges. Thus a port-folio may be made capable of holding the largest cartoons, maps, and prints; and possessing another great advantage, besides that of not bellying or swagging, when laid against a wall, as those constructed of pasteboard do, to their own destruction, and material injury of the things they contain.

A frame of four feet by three will be strong enough, if made of deal. The stiles are four inches wide by half an inch thick; and they have a middle upright stile of the same width, with angle pieces at the corners, as is shown in the engraving, *Fig. 1*. A frame of much greater dimensions may require two middle upright stiles; and, if very large, a middle longitudinal stile, as is shown in the engraving, *Fig. 2*.

If the frames are made of mahogany, they need not exceed 3-16ths of an inch in thickness; but, of whatever wood they are made, it must be well seasoned, or they will warp. A padlock may be applied to such cases, for the protection of their contents.

On this construction I have made two port-folios, one of which I have had in use these ten years. A handsome one, of tolerably large dimensions, I have sent with this paper, for

Method of constructing large port-folios.

the inspection of the Society. The outer stiles of it are of mahogany, which, beaded, forms the out edge in a neat manner. Its simple formation, its lightness, and its firm flatness, must be obvious to every one; and I am of opinion the Society will obtain the thanks of all collectors and artists (if they think it worthy of insertion in the Volume of their Transactions) by making this simple matter publicly known. For my own part, I shall be highly gratified in having contributed to the comfort of artists and collectors, in preserving their valuable researches.

I am, Sir,

Your obedient Servant,

JAMES MALTON.

Norton-street, June 25, 1802.

Reference to the Engraving, Plate VIII.

Fig. 4. One of the sides of the frame for a port-folio, the dimensions four feet by three: it may be made of deal or fir wood. The stiles, four inches wide by half an inch thick; the middle upright stile to be of the same width. It should have angle pieces within the corners, to keep them firm.

Fig. 3. Shows a side of another frame, where much larger dimensions are required; it should then have two upright stiles, and a middle longitudinal stile, all within the frame, and angle pieces at the corners. If it is made of mahogany, instead of deal, the stiles may be reduced nearly one-fourth in breadth and thickness; the wood, in either case, should be well seasoned, that it may not be liable to warp.

XVII.

Experiments and Calculations relative to Physical Optics, By THOMAS YOUNG, M. D. F. R. S. From the Philosophical Transactions for 1804.

(Concluded from p. 64.)

Exp. 2. So likewise the crested fringes of Grimaldi.

Exper. 2. THE crested fringes described by the ingenious and accurate GRIMALDI, afford an elegant variation of the preceding experiment, and an interesting example of a calculation grounded on it. When a shadow is formed by an object which has a rectangular termination, besides the usual external

ternal fringes, there are two or three alternations of colours, beginning from the line which bisects the angle, disposed on each side of it, in curves, which are convex towards the bisecting line, and which converge in some degree towards it, as they become more remote from the angular point. These fringes are also the joint effect of the light which is inflected directly towards the shadow, from each of the two outlines of the object. For, if a screen be placed within a few inches of the object, so as to receive only one of the edges of the shadow, the whole of the fringes disappear. If, on the contrary, the rectangular point of the screen be opposed to the point of the shadow, so as barely to receive the angle of the shadow on its extremity, the fringes will remain undisturbed.

II. COMPARISON OF MEASURES, DEDUCED FROM VARIOUS EXPERIMENTS.

If we now proceed to examine the dimensions of the fringes, Comparison of measures. under different circumstances, we may calculate the differences of the lengths of the paths described by the portions of light, which have thus been proved to be concerned in producing those fringes; and we shall find, that where the lengths are equal, the light always remains white; but that, where either the brightest light, or the light of any given colour, disappears and reappears, a first, a second, or a third time, the differences of the lengths of the paths of the two portions are in arithmetical progression, as nearly as we can expect experiments of this kind to agree with each other. I shall compare, in this point of view, the measures deduced from several experiments of Newton, and from some of my own.

In the eighth and ninth observations of the third book of Newton's Optics, some experiments are related, which, together with the third observation, will furnish us with the data necessary for the calculation. Two knives were placed, with their edges meeting at a very acute angle, in a beam of the sun's light, admitted through a small aperture; and the point of concurrence of the two first dark lines bordering the shadows of the respective knives, was observed at various distances. The results of six observations are expressed in the first three lines of the first Table. On the supposition that the dark line is produced by the first interference of the light reflected from the edges of the knives, with the light passing in a straight line between them,

Comparison of measures. we may assign, by calculating the difference of the two paths, the interval for the first disappearance of the brightest light, as it is expressed in the fourth line. The second Table contains the results of a similar calculation, from Newton's observations on the shadow of a hair; and the third, from some experiments of my own, of the same nature: the second bright line being supposed to correspond to a double interval, the second dark line to a triple interval, and the succeeding lines to depend on a continuation of the progression: The unit of all the Tables is an inch.

TABLE I. *Obs.* 9. N.

Distance of the knives from the aperture	-	-	-	-	-	101.
Distances of the paper from the knives	$1\frac{1}{2}$,	$3\frac{1}{2}$,	$8\frac{3}{4}$,	32,	96,	131.
Distances between the edges of the knives, opposite to the point of concurrence	-	.012,	.020,	.034,	.057,	.081,
Interval of disappearance	.0000122,	.0000155,	.0000182,	.0000167,	.0000166,	.0000166.

TABLE II. *Obs.* 3. N.

Breadth of the hair	-	-	-	-	-	$\frac{1}{280}$.
Distance of the hair from the aperture	-	-	-	-	-	144.
Distances of the scale from the aperture	-	-	-	150,	-	252.
Breadths of the shadow	-	-	-	$\frac{1}{34}$,	-	$\frac{1}{2}$.)
Breadth between the second pair of bright lines	-	-	-	$\frac{2}{47}$,	-	$\frac{4}{17}$.
Interval of disappearance, or half the difference of the paths	-	-	-	-	.0000151,	.0000173.
Breadth between the third pair of bright lines	-	-	-	$\frac{4}{73}$,	-	$\frac{3}{16}$.
Interval of disappearance, $\frac{1}{4}$ of the difference	-	-	-	-	.0000130,	.0000143.

TABLE III. *Exper.* 3.

Breadth of the object	-	-	-	-	-	.434,
Distance of the object from the aperture	-	-	-	-	-	125.
Distance of the wall from the aperture	-	-	-	-	-	250.
Distance of the second pair of dark lines from each other	-	-	-	-	-	1.167.
Interval of disappearance, $\frac{1}{2}$ of the difference	-	-	-	-	-	.0000149.

Exper.

Exper. 4.

Breadth of the wire	-	-	-	-	.083.
Distance of the wire from the aperture	-	-	-	-	32.
Distance of the wall from the aperture	-	-	-	-	250.
(Breadth of the shadow by three measurements	-	-	-	.815, .826, or .827; mean, .823.)	
Distance of the first pair of dark lines	-	-	-	-	1.165, 1.170, or 1.160; mean, 1.165.
Interval of disappearance	-	-	-	-	.0000194.
Distance of the second pair of dark lines	-	-	-	-	1.402, 1.395, or 1.400; mean, 1.399.
Interval of disappearance	-	-	-	-	.0000137.
Distance of the third pair of dark lines	-	-	-	-	1.594, 1.580, or 1.585; mean, 1.586.
Interval of disappearance	-	-	-	-	.0000128.

It appears, from five of the six observations of the first Table, in which the distance of the shadow was varied from about 3 inches to 11 feet, and the breadth of the fringes was increased in the ratio of 7 to 1, that the difference of the routes constituting the interval of disappearance, varied but one-eleventh at most; and that, in three out of the five, it agreed with the mean, either exactly, or within $\frac{1}{11}$ part. Hence we are warranted in inferring, that the interval appropriate to the extinction of the brightest light, is either accurately or very nearly constant.

But it may be inferred, from a comparison of all the other observations, that when the obliquity of the reflection is very great, some circumstance takes place, which causes the interval thus calculated to be somewhat greater: thus, in the eleventh line of the third Table, it comes out one-sixth greater than the mean of the five already mentioned. On the other hand, the mean of two of Newton's experiments and one of mine, is a result about one-fourth less than the former. With respect to the nature of this circumstance, I cannot at present form a decided opinion; but I conjecture that it is a deviation of some of the light concerned, from the rectilinear direction assigned to it, arising either from its natural diffraction, by which the magnitude of the shadow is also enlarged, or from some other unknown cause. If we imagined the shadow of the wire, and the

Comparison of
measures.

fringes nearest it, to be so contracted that the motion of the light bounding the shadow might be rectilinear, we should thus make a sufficient compensation for this deviation; but it is difficult to point out what precise tract of the light would cause it to require this correction.

The mean of the three experiments which appear to have been least affected by this unknown deviation, gives .0000127 for the interval appropriate to the disappearance of the brightest light; and it may be inferred, that if they had been wholly exempted from its effects, the measure would have been somewhat smaller. Now the analogous interval, deduced from the experiments of Newton on thin plates, is .0000112, which is about one-eighth less than the former result; and this appears to be a coincidence fully sufficient to authorise us to attribute these two classes of phenomena to the same cause. It is very easily shown, with respect to the colours of thin plates, that each kind of light disappears and reappears, where the differences of the routes of two of its portions are in arithmetical progression; and we have seen, that the same law may be in general inferred from the phenomena of diffracted light, even independently of the analogy.

The distribution of the colours is also so similar in both cases, as to point immediately to a similarity in the causes. In the thirteenth observation of the second part of the first book, Newton relates, that the interval of the glasses where the rings appeared in red light, was to the interval where they appeared in violet light, as 14 to 9; and, in the eleventh observation of the third book, that the distances between the fringes, under the same circumstances, were the 22d and 27th of an inch. Hence, deducting the breadth of the hair, and taking the squares, in order to find the relation of the difference of the routes, we have the proportion of 14 to $9\frac{1}{4}$, which scarcely differs from the proportion observed in the colours of the thin plate.

We may readily determine, from this general principle, the form of the crested fringes of Grimaldi, already described; for it will appear that, under the circumstances of the experiment related, the points in which the differences of the lengths of the paths described by the two portions of light are equal to a constant quantity, and in which, therefore, the same kinds of light ought to appear or disappear, are always found in equilateral hyperbolas, of which the axes coincide with the outlines of the shadow,

shadow, and the asymptotes nearly with the diagonal line. Such, therefore, must be the direction of the fringes; and this conclusion agrees perfectly with the observation. But it must be remarked, that the parts near the outlines of the shadow, are so much shaded off, as to render the character of the curve somewhat less decidedly marked where it approaches to its axis. These fringes have a slight resemblance to the hyperbolic fringes observed by Newton; but the analogy is only distant.

III. APPLICATION TO THE SUPERNUMERARY RAINBOWS.

The repetitions of colours sometimes observed within the common rainbow, and described in the Philosophical Transactions, by Dr. Langwith and Mr. Daval, admit also a very easy and complete explanation from the same principles. Dr. Pemberton has attempted to point out an analogy between these colours and those of thin plates; but the irregular reflection from the posterior surface of the drop, to which alone he attributes the appearance, must be far too weak to produce any visible effects. In order to understand the phenomenon, we have only to attend to the two portions of light which are exhibited in the common diagrams explanatory of the rainbow, regularly reflected from the posterior surface of the drop, and crossing each other in various directions, till, at the angle of the greatest deviation, they coincide with each other, so as to produce, by the greater intensity of this redoubled light, the common rainbow of 41 degrees. Other parts of these two portions will quit the drop in directions parallel to each other; and these would exhibit a continued diffusion of fainter light, for 25° within the bright termination which forms the rainbow, but for the general law of interference, which, as in other similar cases, divides the light into concentric rings; the magnitude of these rings depending on that of the drop, according to the difference of time occupied in the passage of the two portions, which thus proceed in parallel directions to the spectator's eye, after having been differently refracted and reflected within the drop. This difference varies at first, nearly as the square of the angular distance from the primitive rainbow: and, if the first additional red be at the distance of 2° from the red of the rainbow, so as to interfere a little with the primitive violet, the fourth additional red will be at a distance of nearly 2° more;

Application of
the doctrine of
the interference
of light to the
supernumerary
rainbows of
Langwith and
Daval.

more; and the intermediate colours will occupy a space nearly equal to the original rainbow. In order to produce this effect, the drops must be about $\frac{1}{70}$ of an inch, or .013, in diameter: it would be sufficient if they were between $\frac{1}{70}$ and $\frac{1}{80}$. The reason that such supernumerary colours are not often seen, must be, that it does not often happen that drops so nearly equal are found together: but, that this may sometimes happen, is not in itself at all improbable: we measure even medicines by dropping them from a phial, and it may easily be conceived that the drops formed by natural operations may sometimes be as uniform as any that can be produced by art. How accurately this theory coincides with the observation, may best be determined from Dr. Langwith's own words.

“ August the 21st, 1722, about half an hour past five in the evening, weather temperate, wind at north-east, the appearance was as follows: The colours of the primary rainbow were as usual, only the purple very much inclining to red, and well defined: under this was an arch of green, the upper part of which inclined to a bright yellow, the lower to a more dusky green: under this were alternately two arches of reddish purple, and two of green: under all, a faint appearance of another arch of purple, which vanished and returned several times so quick, that we could not readily fix our eyes upon it. Thus the order of the colours was, I. Red, orange-colour, yellow, green, light blue, deep blue, purple. II. Light green, dark green, purple. III. Green, purple. IV. Green, faint vanishing purple. You see we had here four orders of colours, and perhaps the beginning of a fifth; for I make no question but that what I call the purple, is a mixture of the purple of each of the upper series with the red of the next below it, and the green a mixture of the intermediate colours. I send you not this account barely upon the credit of my own eyes; for there was a clergyman and four other gentlemen in company, whom I desired to view the colours attentively, who all agreed, that they appeared in the manner that I have now described. There are two things which well deserve to be taken notice of, as they may perhaps direct us, in some measure, to the solution of this curious phenomenon. The first is, that the breadth of the first series so far exceeded that of any of the rest, that, as near as I could judge, it was equal to them all taken together. The second is, that I have never observed these inner orders
of

of colours in the lower parts of the rainbow, though they have often been incomparably more vivid than the upper parts, under which the colours have appeared. I have taken notice of this so very often, that I can hardly look upon it to be accidental; and, if it should prove true in general, it will bring the disquisition into a narrow compass; for it will show that this effect depends upon some property which the drops retain, whilst they are in the upper part of the air, but lose as they come lower, and are more mixed with one another." Phil. Transf. Vol. XXXII. p. 243.

From a consideration of the nature of the secondary rainbow, of 54° , it may be inferred, that if any such supernumerary colours, were seen attending this rainbow, they would necessarily be external to it, instead of internal. The circles sometimes seen encompassing the observer's shadow in a mist, are perhaps more nearly related to the common colours of thin plates as seen by reflection.

IV. ARGUMENTATIVE INFERENCE RESPECTING THE NATURE OF LIGHT.

The experiment of Grimaldi, on the crested fringes within the shadow, together with several others of his observations, equally important, has been left unnoticed by Newton. Those who are attached to the Newtonian theory of light, or to the hypotheses of modern opticians, founded on views still less enlarged, would do well to endeavour to imagine any thing like an explanation of these experiments, derived from their own doctrines; and, if they fail in the attempt, to refrain at least from idle declamation against a system which is founded on the accuracy of its application to all these facts; and to a thousand others of a similar nature.

From the experiments and calculations which have been premised, we may be allowed to infer, that homogeneous light, at certain equal distances in the direction of its motion, is possessed of opposite qualities, capable of neutralizing or destroying each other, and of extinguishing the light, where they happen to be united; that these qualities succeed each other alternately in successive concentric superficies, at distances which are constant for the same light, passing through the same medium. From the agreement of the measures, and from the similarity of the phenomena, we may conclude, that

Argumentative inferences.

Homogeneous light has opposite qualities at equal distances along its course, by which neutralization or extinction of the light may be effected.

Light moves
more slowly in
denser mediums.

Light and sound
strongly resemble
each other.

There is prob-
ably no inflect-
ing medium.

Practical appli-
cation of the
facts.

that these intervals are the same as are concerned in the production of the colours of thin plates; but these are shown, by the experiments of Newton, to be the smaller, the denser the medium; and, since it may be presumed that their number must necessarily remain unaltered in a given quantity of light, it follows of course, that light moves more slowly in a denser, than in a rarer medium: and this being granted, it must be allowed, that refraction is not the effect of an attractive force directed to a denser medium. The advocates for the projectile hypothesis of light, must consider which link in this chain of reasoning they may judge to be the most feeble; for, hitherto, I have advanced in this Paper no general hypothesis whatever. But, since we know that sound diverges in concentric superficies, and that musical sounds consist of opposite qualities, capable of neutralising each other, and succeeding at certain equal intervals, which are different according to the difference of the note, we are fully authorised to conclude, that there must be some strong resemblance between the nature of sound and that of light.

I have not, in the course of these investigations, found any reason to suppose the presence of such an inflecting medium in the neighbourhood of dense substances as I was formerly inclined to attribute to them; and, upon considering the phenomena of the aberration of the stars, I am disposed to believe, that the luminiferous ether pervades the substance of all material bodies with little or no resistance, as freely perhaps as the wind passes through a grove of trees.

The observations on the effects of diffraction and interference, may perhaps sometimes be applied to a practical purpose, in making us cautious in our conclusions respecting the appearances of minute bodies viewed in a microscope. The shadow of a fibre, however opaque, placed in a pencil of light admitted through a small aperture, is always somewhat less dark in the middle of its breadth than in the parts on each side. A similar effect may also take place, in some degree, with respect to the image on the retina, and impress the sense with an idea of a transparency which has no real existence: and, if a small portion of light be really transmitted through the substance, this may again be destroyed by its interference with the diffracted light, and produce an appearance of partial opacity, instead of uniform semitransparency.

parency. Thus, a central dark spot, and a light spot surrounded by a darker circle, may respectively be produced in the images of a semitransparent and an opaque corpuscle; and impress us with an idea of a complication of structure which does not exist. In order to detect the fallacy, we may make two or three fibres cross each other, and view a number of globules contiguous to each other; or we may obtain a still more effectual remedy by changing the magnifying power; and then, if the appearance remain constant in kind and in degree, we may be assured that it truly represents the nature of the substance to be examined. It is natural to inquire whether or no the figures of the globules of blood, delineated by Mr. Hewson in the Phil. Transf. Vol. LXIII. for 1773, might not in some measure have been influenced by a deception of this kind: but, as far as I have hitherto been able to examine the globules, with a lens of one-fiftieth of an inch focus, I have found them nearly such as Mr. Hewson has described them.

REMARKS ON THE COLOURS OF NATURAL BODIES.

Exper. 5. I have already adduced, in illustration of Newton's comparison of the colours of natural bodies with those of thin plates, Dr. Wollaston's observations on the blue light of the lower part of a candle, which appears, when viewed through a prism, to be divided into five portions. I have lately observed a similar instance, still more strongly marked, in the light transmitted by the blue glass fold by the opticians. This light is separated by the prism into seven distinct portions, nearly equal in magnitude, but somewhat broader, and less accurately defined, towards the violet end of the spectrum. The first two are red, the third is yellowish green, the fourth green, the fifth blue, the sixth bluish violet, and the seventh violet. This division agrees very nearly with that of the light reflected by a plate of air $\frac{1}{1575}$ of an inch in thickness, corresponding to the 11th series of red and the 18th of violet. A similar plate of metallic oxide, would perhaps be about $\frac{1}{15000}$ of an inch in thickness. But it must be confessed, that there are strong reasons for believing the colouring particles of natural bodies in general to be incomparably smaller than this; and it is probable that the analogy, suggested by Newton, is somewhat less close than he

he imagined. The light reflected by a plate of air, at any thickness nearly corresponding to the 11th red, appears to the eye to be very nearly white; but, under favourable circumstances, the 11th red and the neighbouring colours may still be distinguished. The light of some kinds of coloured glass is pure red; that of others, red with a little green: some intercept all the light, except the extreme red and the blue. In the blue light of a candle, expanded by the prism, the portions of each colour appear to be narrower, and the intervening dark spaces wider, than in the analogous spectrum derived from the light reflected from a thin plate. The light of burning alcohol appears to be green and violet only. The pink dye sold in the shops, which is a preparation of the carthamus, affords a good specimen of a yellow green light regularly reflected, and a crimson probably produced by transmission.

VI. EXPERIMENT ON THE DARK RAYS OF RITTER.

Dark rays of
Ritter observed
also by Dr.
Wollaston.

Exper. 6. The existence of solar rays accompanying light, more refrangible than the violet rays, and cognisable by their chemical effects, was first ascertained by Mr. Ritter: but Dr. Wollaston made the same experiments a very short time afterwards, without having been informed of what had been done on the Continent. These rays appear to extend beyond the violet rays of the prismatic spectrum, through a space nearly equal to that which is occupied by the violet. In order to complete the comparison of their properties with those of visible light, I was desirous of examining the effect of their reflection from a thin plate of air, capable of producing the well known rings of colours. For this purpose, I formed an image of the rings, by means of the solar microscope, with the apparatus which I have described in the Journals of the Royal Institution, and I threw this image on paper dipped in a solution of nitrate of silver, placed at the distance of about nine inches from the microscope. In the course of an hour, portions of three dark rings were very distinctly visible, much smaller than the brightest rings of the coloured image, and coinciding very nearly, in their dimensions, with the rings of violet light that appeared upon the interposition of violet glass. I thought the dark rings were a little smaller than the violet rings, but the difference was not sufficiently great to be accurately ascertained; it might be as much as $\frac{1}{30}$ or $\frac{1}{40}$ of the diameters, but not greater. It is

They are reflected from a thin plate of air with the same modification of rings as visible light is.

is the less surprising that the difference should be so small, as the dimensions of the coloured rings do not by any means vary at the violet end of the spectrum, so rapidly as at the red end. For performing this experiment with very great accuracy, a helioslate would be necessary, since the motion of the sun causes a slight change in the place of the image; and leather, impregnated with the muriate of silver, would indicate the effect with greater delicacy. The experiment, however, in its present state, is sufficient to complete the analogy of the invisible with the visible rays, and to show that they are equally liable to the general law which is the principal subject of this Paper. If we had thermometers sufficiently delicate, it is probable that we might acquire, by similar means, information still more interesting, with respect to the rays of invisible heat discovered by Dr. Herschel; but at present there is great reason to doubt of the practicability of such an experiment.

SCIENTIFIC NEWS, ACCOUNT OF BOOKS, &c.

National Institute.

THE class of mathematical and physical sciences of the National Institute had a public sitting on the 6th Messidor (June 25.) The subjects appointed for prizes were the following:

Mathematics. "It is required to give a theory of the perturbations of the planet Pallas, discovered by M. Olbers."

The prize will be a gold medal, weighing one kilogramme, (35½ oz. avoirdupois.)

Natural Philosophy. The class had proposed for the subject of a prize the following question. "To determine by experiment the different sources of carbon in vegetables." The concurrence is prorogued until the 1st Germinal in the year 13 (March 22, 1805.)

The concurrence for the prize upon the following question is also prolonged to the same period. "To determine by anatomical and chemical observations and experiments, what are the phenomena of the torpid state to which certain animals, such as marmots, dormice, &c. are subject during the winter, with respect to the circulation of the blood, respiration, and irritability; to investigate the causes of this sleep, and why it is peculiar to these animals."

The

The value of these two prizes is doubled, and consists in two kilogrammes of gold about 6800 francs each (£289.)

The class had proposed for the second time, the 15th Germinal of the year 10, as the subject of a prize to be decreed in the public sitting of Messidor, the following question.

Ferments.

"What are the characters which distinguish, in vegetable and animal matters, those bodies which serve as ferments from those in which they produce the state of fermentation."

The memoirs received not having answered the conditions of the program, and the class considering that this question has been four years before the public, the subject is now withdrawn.

**Astronomy.
Prize gained by
Piazzi.**

Astronomy. Mr. Piazzi of Palermo has obtained the prize founded by M. De Lalande, in favour of the person who shall have made the most interesting observation, or written the most useful memoir to the progress of astronomy. Mr. Piazzi is the discoverer of the planet Ceres.

Readings.

The readings delivered during the sitting were as follows:

1. Notice of the labours of the class during the year; the mathematical part by M. Delambre. 2. Notice of their labours; the physical part by M. Cuvier. 3. Account of a physico-mathematical theory of currents of water, by M. Prony. 4. A note respecting the amelioration of sheep in the south of France, by M. Tessier. 5. General reflections on the productions of the vegetable kingdom in the Pyrenean mountains by M. Raymond. 6. Reflections on heat, by Count Rumford. 7. Extract of a memoir tending to illustrate the æconomical history of vegetables, cultivated or naturally growing in the Canaries, by M. Broussonet.

*Extract of a Letter from PROFESSOR BODE, Astronomer Royal,
to Mr. A. F. Thødden.*

Dated Berlin, Sept. 18.

**Place of new
planets.**

ON the first of September Mr. Harding discovered, at Lilienthal, a new moveable star; it appears to be of the eighth magnitude; its motion retrograde towards the South. Probably this may be another new planet in the orbit of Ceres and Pallas.

1st Sept.

1st Sept. 10 ^h .—Even.	R.A. 2° 24'	Decl. 0° 37'—N.
5 — 11 8'	- 1 51 51"	- 0 11 26" S.
8 — 8 11	- 1 29 28	- 0 47 19 -
10 — 8 15	- 1 12 55	- 1 11 55 -

The two last observations were made by Dr. Olbers, at Bremen. I received this intelligence yesterday, but at present this small planet (there are now three) is not discoverable, on account of the moon-shine.

Extract of a Letter from Mr. G. B. GRENOUGH.

* * * There is an oversight in the hasty account I sent you of the Abbé Melograni's blow-pipe. No provision is made for admitting the external air. This may be done by several means. It may be admitted through that part of the axis which is opposite the nozzle; but as this would require some contrivance which would render it defective in simplicity, I should prefer the addition of a small valve opening inwards at that part of each vessel which is immediately opposite the neck that joins them. Whether this method was used in the actual instrument I cannot, at this distance of time, recollect.

Organic Remains of a former World. An Examination of the Mineralized Remains of the Vegetables and Animals of the Antediluvian World, generally termed extraneous Fossils. By JAMES PARKINSON, Hoxton. Vol. I. containing the Vegetable Kingdom. 4to. pp. 471. with 10 Plates (£2 2 0) Robson, &c.

AS it would be impracticable in the general announcement of a work of the magnitude and importance of the present to give the reader any well filled outline of the plan, I must confine myself to state in few words, that the description and elucidation of organic fossil bodies, though in many points of view of the highest interest, has not yet constituted the subject of a general treatise; notwithstanding the various considerations respecting the local, the individual, and the chemical history of these wonderful bodies have occupied the researches of many able philosophers in detached memoirs; and that the composition of such an history demands a great exertion of labour, in consulting and digesting what has been

done.

Parkinson's organic remains.

done by others, an accurate, geological, and chemical view of the facts upon which scientific conclusions or deductions may be grounded, and above all, an ardent curiosity, with adequate leisure and means to become acquainted with the specimens themselves:—And, after this preface, I must add, that fortunately for the advancement of this branch of knowledge, the author of the present work has amply vindicated his claim to these requisites.

The work is written in the form of letters, and enlivened by an adoption of the style and some of the incidents of a traveller. Much of useful science and clear explanations of the subject are to be found in the pages of this volume. The abridgements from other authors are full, and the quotations always given with the volume and page, which I mention more readily, as a slovenly habit of loose quotation, or not quoting at all, is but too common, even with our writers of merit. Nine plates of the subjects are given, which do the highest credit to the designer and engraver, Springsguth, for their delicacy and beauty, and considerable pains have been taken in colouring them. The frontispiece, representing the Ark upon Mount Ararat, designed by Corbould, is happy and appropriate; though the artist has taken a pictorial licence to represent the sun and the rainbow nearly on the same azimuth, for which neither opticians nor correct observers will be disposed to excuse him.

The volume yet to be published will contain the animal kingdom; a subject perhaps still more attractive than the vegetable. In the remains of extinct vegetables, we appear to contemplate the physical economy of a world long ago obliterated; but in those of animals, we seem to behold something that gives a glimpse into the moral relations of those beings of past ages who existed upon its surface.

*Apparatus for Filtration
by Mr. H. C. Englefield Bar. & Co.*

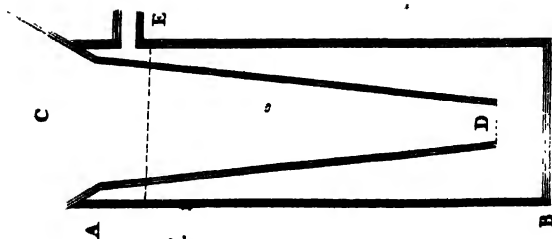


Fig. 1

*Structure for purifying a Stream
of Water*

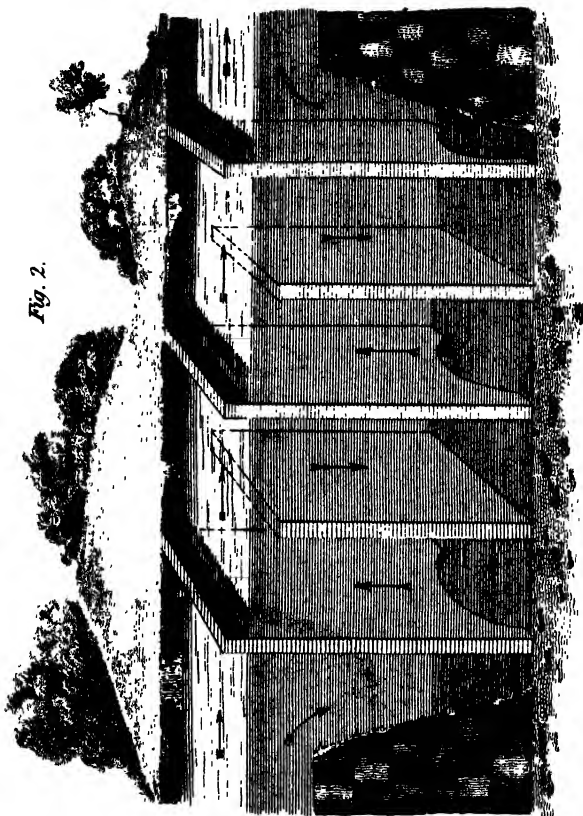
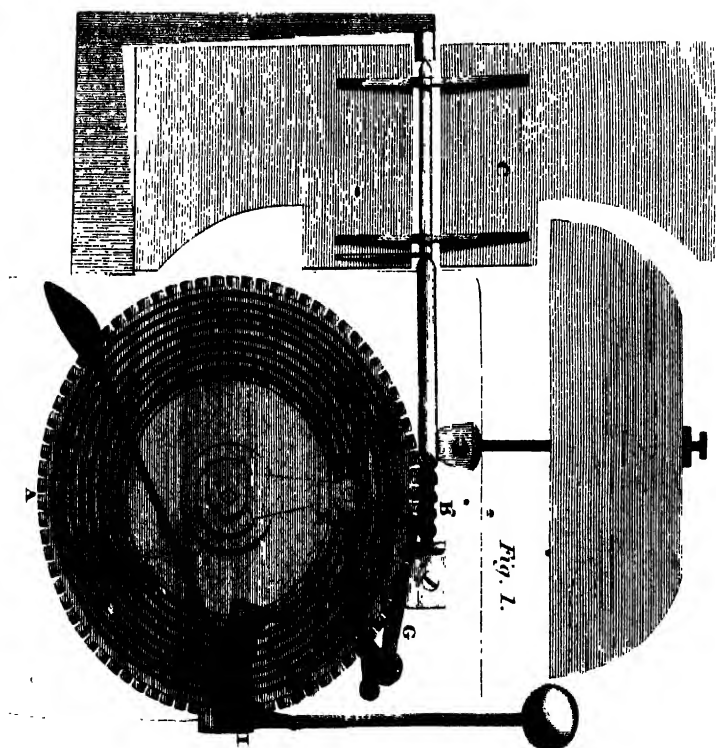
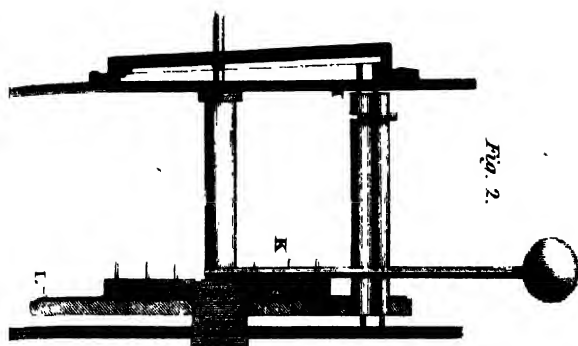


Fig. 2.

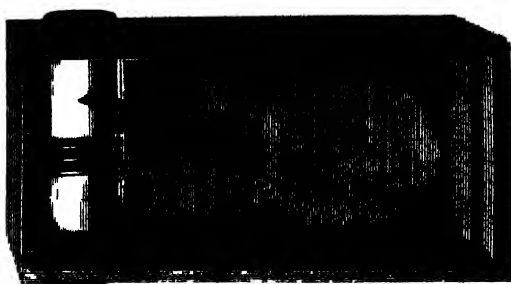
Drawn by Shaw

Engineered by Mather.

Working part of a clock by W. Davis.

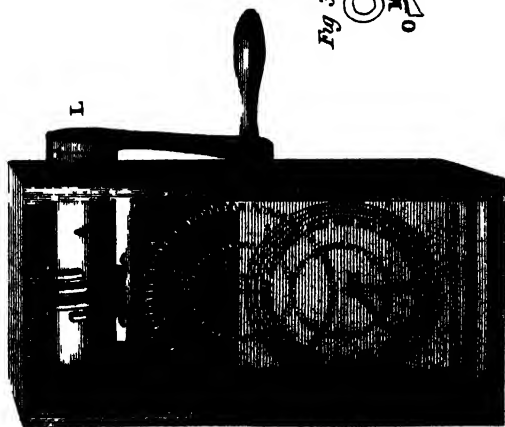


Mr. Arthur. Indicator of the number of draughts from a c. line.



L

Fig. 1.



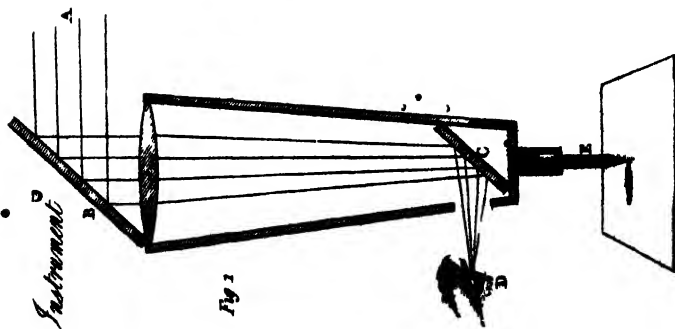
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Fig. 2.

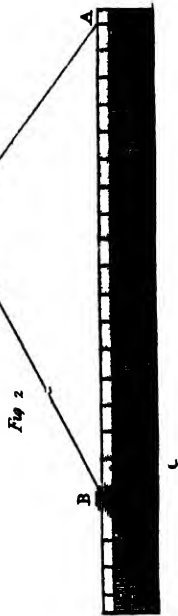


Fig. 3.

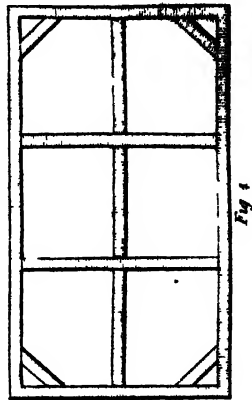
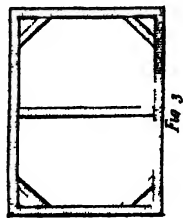
Perspective Instrument



Instrument for drawing Levels



Mallons Port-folios



A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

NOVEMBER, 1804.

• ARTICLE I.

Description of a Tallow Lamp, which regulates its Supply by a spontaneous Movement. By Mr. JOHN WHITLEY BOSWELL. Communicated by the Inventor.

To Mr. NICHOLSON.

SIR,

THE constant attention which candles demand from the frequent snuffing they require, and the continued variation of their light in the intervals of this operation, on the one hand, and on the other the expence and trouble which oil lamps occasion, together with the disagreeable scent they in general produce, have rendered the contrivance of a good lamp for burning tallow, a desirable object to several for a long time.

I hope, therefore, the description I send of a lamp for this purpose, which possesses some valuable properties, will be acceptable to you.

The best lamp for burning tallow which I had seen before I contrived this, was one invented by Mr. March, of Barnstaple, in which the tallow kept continually melted in a vessel suspended over the flame, was admitted by a small pipe to the burner, and the supply regulated by a kind of cock attached

Description of a
lamp for burning
tallow.

to the pipe: But this method occasioned a waste from the evaporation of the melted tallow, and besides, required much attendance to regulate the cock and keep the tallow vessel at a proper distance from the flame; for the alteration of the temperature of the melted tallow, which was very frequent from many causes, continually changing its degree of fluidity, caused it to vary in its supply through the pipe proportionally.

It occurred to me some time after I saw this lamp, that one might be contrived in which the tallow should only be melted as it was consumed, and other of the inconveniences be removed to which the first was subject; and I made then a rough drawing of a lamp for this purpose, but other more interesting matters prevented my having one made on that plan before last December: It consisted of a trough supported in an inclination of about 45 degrees, similar to the one in the figure, which held a square prism of tallow; under this was placed a little oblong vessel of tin, formed so as to move forwards and backwards like a drawer, in front of which was placed a burner: a couple of wires placed across the lower part of the trough, prevented the tallow from sliding down beyond them, as it otherwise would from the obliquity of its support; but as it melted by the heat from the burner, it fell in drops into the little drawer, so as to supply the flame, while the degree of this supply was regulated by moving the drawer in or out, so as to bring the burner nearer to, or further from the tallow. In this state, though it formed a very useful lamp, it was not so perfect as I could wish; every alteration of temperature of the apartment (which in cold weather happens frequently) required a proportional alteration in the distance of the burner from the tallow, to prevent the little drawer being either overflowed by a superabundance of melted tallow, or the flame ceasing from a deficiency of supply. The attendance this circumstance required induced me to contrive the present lamp, or rather to improve the former, in manner following.

I conceived that if the little pan, which held the melted tallow and the burner, was placed on the arm of a balance inclined from 30 to 45 degrees from the perpendicular, and its weight compensated by a counterpoise at the extremity of the other arm, that when more than a certain quantity of tal-
low

low pan into it, it would sink lower, and, in doing so, move the burner farther forward from the trough at the same time, and *vice versa*, and thus regulate the supply spontaneously: But as it was necessary that the pan (holding a fluid) should always maintain an horizontal position during its movement, I imagined this might be effected by placing another arm beneath the balance-arm, and fixing a perpendicular piece, so jointed to the extremity of each as to move fresh and steadily, which piece should support an horizontal stand for the pan.

I had a lamp made in this manner, and found it answer my intention perfectly. It required no attendance whatsoever, but regulated its supply with precision; and afforded, likewise, an agreeable spectacle, having in its movements somewhat the air of those of an animal, from their exact relation to an evident object, and adopting themselves to all its changes.

The lamp thus made assumed the form in the figure, Pl. IX. where T represents the inclined trough for holding the tallow; P the pan for catching the tallow, as it melted, and holding the burner; F the flame and burner; R the perpendicular piece sustaining a support for the pan; B the balance moving freely on its center in the stand S, and jointed to R as before described; c the parallel arm used to keep R perpendicular, and by its means horizontal; w the counter weight, placed so as to move on the extremity of B by a screw. The first frosty night shewed me the necessity of the addition of another part to remedy an inconvenience I had not foreseen; the dropping tallow assuming the form of an icicle, joined the pan to the trough, and thus stopped its movement; *A, it was troublesome to break this off as it occurred, I contrived the following means to prevent its formation: A little oblong sliding-piece, the same breadth as the trough, was placed close underneath its lower part; the extremity of this slide was made angular, that the melted tallow might fall from a single point; a wire represented at L, moved it back or forwards as required: By this slide the drop may be made to fall as close to the stream of burning air from the flame as is thought fit, and all congelation prevented. I afterwards added a sort of reflector to the lamp, which considerably improved the light, not merely acting as its name implies, but also occasioning an

Description of a
lamp for burning
tallow.

increase in the current of air which passed the flame, rendered it somewhat more bright, and considerably more steady.

This part is represented at D in the figure, and consists of two side-pieces placed before the trough, but so as not to obstruct the tallow, and bent forward to form an angle with its sides. To improve the light, I also use in the lamp five small distinct wicks, of three threads each, instead of one large wick, placed near each other in a row, which renders the flame clear and free from smoke, and thereby also prevents any unpleasant scent. I used this lamp for more than three months, and found it very convenient for reading or writing by, as when once lighted it required no farther attention, and kept its light at nearly the same height at all times, and of the same degree of intensity; and I can further recommend it, as yielding the greatest light at the smallest cost, in proportion, of any invention yet made public which is applicable to domestic uses.

I have had two lamps of this kind carefully made by Mr. Lloyd, No. 179, Strand, and left with him as models: In one of which the chief object is cheapness, as far as consistent with perfection and neatness; in the other, to form an elegant utensil for the study, office, or chamber; and any gentleman may be supplied with them there, made exactly to my plan, on reasonable terms; and as Mr. Lloyd is the only person at present appointed to make them, and instructed fully in the mechanism, they can be had perfect from him only.

It is probable wax might also be burned in this lamp as well as tallow: of this I cannot speak from experience, but mention it, as I know some would prefer to use it.

I am, Sir,

Your very humble servant,

J. WHITLEY BOSWELL.

OS. 1, 1804.

II.

Account of a successful Case in which Deafness was cured by Puncture of the Membrana Tympani. By M. Maunoir, of Geneva. Communicated by ALEXANDER MARCET, M.D. Physician to Guy's Hospital.

To Mr. NICHOLSON.

SIR,

THE inclosed communication on the effects produced in a case of deafness by the perforation of the tympanum of the ear, is extracted from a letter which I have just received from Mr. Maunoir, an eminent surgeon at Geneva. The history of this case, drawn up by such an intelligent and accurate observer, appears to me curious, both from the physiological remarks which it contains, and from the striking illustration which it affords of a new mode of practice lately proposed in this country. And, as this subject seems fully as interesting in its philosophical point of view, as in its connection with practical surgery, I should wish, if you think it proper, that you would give it a place in the next number of your valuable Journal. I have the honour to be, Sir,

Your obedient humble servant,

ALEXANDER MARCET.

St. Mary Axe, Oct. 16, 1804.

Extract of a Letter from Mr. Maunoir, surgeon at Geneva, dated 5th September, 1804.

“ Mr. F. of Geneva, aged forty years, had lost his hearing, several years ago, in consequence of a long affection of the posterior part of the internal fauces, and it was evident, that, in this case, the eustachian tubes were obliterated. In condensing the air in his mouth and nostrils, he was totally unable to distend the membrane of the tympanum, a thing which is very easy to do for any one who enjoys a perfect sense of hearing. He could scarcely hear, even when you shouted in his ears, and although so long deaf, he had not acquired the faculty of understanding by the motion of the lips. After perusing the paper which I lately published in the *Bibliothèque Britannique*, Mr. F. conceived that he might be one of those in whom the hearing could be recovered by the perforation of the

A gentleman
aged 40 years
had been several
years extremely
deaf.

Puncture of the
tympanum of
one ear.

Instant restora-
tion of hearing
with extreme
sensibility.

Some sounds
more audible
than others.

The other ear
punctured with-
out effect.

Cause of this
failure.

On the 14th of July last, I performed that operation on his right ear, in presence of Mr. Jurine, with a very small trocar, about one eighth of an inch in diameter. At the instant that the instrument was withdrawn, we spoke to him in a low voice; but instead of answering, he remained immoveable in his chair, and seemed stupified.—Then he exclaimed “For God’s sake gentlemen, don’t cry out so loud; you hurt me!” I then began to walk about the room,—The squeaking of my boots made him shudder and jump in his chair; and he covered his ear with his hand. The snapping of the fingers quite distracted him, and seemed to produce upon him an effect similar to that of a pistol suddenly fired in the ear of a person not aware of it. When spoken to, though in a whisper, he complained that we spoke too loud. Yet in spite of this excessive sensibility to very inconsiderable noises, there were other sounds, not less distinct, which he did not hear, even at a very little distance; such for instance as the *ticking* of his watch. So that his hearing seemed to be sometimes too dull, sometimes too acute; because, no doubt, the organ had lost the faculty of adjusting itself to the different modulations of sound. A week after this, however, Mr. F. had lost that excessive sensibility which rendered acute sounds almost intolerable to him, and in fact he had already *again learnt* to hear. He then wished to have the other ear perforated in the same manner. I performed this operation, but without effect.—Twenty days after this second operation, Mr. F. called upon me. I examined his ears in a fine sun shine, and could see in the right ear the membrane of the tympanum traversed in its exterior part by a small cicatrix, having in its center a small hole which was hardly perceptible. Yet the hearing was scarcely diminished since the first operation. Fearing that this small aperture might close itself entirely, Mr. F. desired that I would perforate again the membrane of that ear, an operation which was repeated without giving him any pain, but not without a slight increase of sensibility in his hearing. I afterwards examined the left ear, and having placed the meatus in the direction of the sun’s rays. I distinctly perceived a false membrane adhering to the whole circumference of the passage, and perfectly similar, in its form and situation, to that of the tympanum, the distance between the two membranes being scarcely one sixth part of an inch. I took off at once this

false membrane with a pair of tweezers, and discovered behind it, the tympanum, quite sound and entire. I thought it probable that, in the second operation, I had only touched this false membrane. I immediately perforated the true one, and was surprized (as well as my brother who attended this operation), to find that, although Mr. F. had already learnt again to hear with the right ear, yet the restoration of his hearing in the left, occasioned in him the same effects of wonder, and of excessive sensibility to the least unexpected noise, which he had experienced after the first operation. This inconvenience, however, has not been of longer duration in this ear, than it had been in the other.”

Repetition of the operation with effect and similar phenomena.

III.

Letter from Mr. TIMOTHY SHELDRAKE, exposing the Errors of M. Tingry respecting Copal Varnishes; with some farther Instructions concerning the same.

(Received Oct. 16, 1804.)

To Mr. NICHOLSON.

SIR,

M. TINGRY, professor of chemistry, &c. in the academy of Geneva, has published a treatise on the art of making and applying varnishes; with new observations and experiments on copal, &c. &c. A translation of that work has lately appeared here, and at p. 157 of that translation I find the following passage.

Tingry in his art of making varnishes—

“ Mr. Timothy Sheldrake mentions camphor as a medium for dissolving copal in essence* and alcohol. He gives also another process, in which ammonia (volatile alkaline spirit of sal ammoniac) in the proportion of an eighth part of the essence employed, is substituted in the room of camphor.

“ *Exper. I.* Of the three processes which he describes, I repeated two: those which regard the solution of copal in essence and in alcohol by a mixture of camphor. *That with essence did not succeed.* The author himself announces that he

I repeated Sheldrake's process for dissolving copal in essence and did not succeed;—

* Throughout the book this word is used to describe what we call spirit of turpentine.

always

always failed, except when he obtained the essence from apothecaries hall. It appears that this essence had by chance all those essential qualities which we endeavour to give it by time, and still more speedily by the influence of light.

nor with alcohol. "Exp. II. The same experiment repeated with pure alcohol was attended with too little success to make the result be considered as a varnish. The alcohol appeared milky, and the copal formed at the bottom of the vessel a mass which did not seem to have decreased in volume. Next morning the interposed part of the copal, which altered the limpidity of the alcohol, was precipitated, and adhered to the sides of the glass. The process I have described for spirituous tincture of amber would give more hopes of success without any intermediate substance."

He infers that the compounds must be imperfect and not varnishes.

"In regard to the means proposed by the medium of ammonia, the saline nature of that liquid, if the process succeeds, will not admit of the product being placed in the class of varnishes destined for delicate painting. It is a kind of saponaceous compound, the use of which is not to be recommended in such cases."

Observations in reply.

The passage above quoted undoubtedly relates to a paper which I communicated several years ago to the Society for the Encouragement of Arts, &c.

This paper was wholly transcribed * into some periodical publications, and parts of it into others; it may therefore be necessary to observe that when reference is made to the original paper, it must be understood to mean as it was printed in the Society's Transactions, and not to any imperfect extracts that may have been made from it.

Mr. Tingry ought not to have opposed speculation against facts.

Mr. Tingry should have been sensible of the danger in opposing a conjecture or opinion purely speculative, to what is described as a fact that had been demonstrated by experiment. Since it is more probable that the conjecture should be unfounded, or the opinion untrue, than that a man should forfeit his reputation with the public by relating that, as having been demonstrated by experiment, which the person who contradicts him says cannot, in the nature of things be true, even if the process succeeds: As it is the intention of your valuable Journal to promote the cause of science, and as that cause will be as effect-

* See our Journal, I. 250.

unperserved by the detection of errors, as by the promulgation of new facts, I trust you will favour me by inserting the following observations on those passages which I have quoted from Mr. Tingry's work; beginning with that in which he attempts to demolish one part of discovery (if it may be so called) without using one experiment as the instrument of destruction.

"In regard to the means proposed by the medium of ammonia, the saline nature of that liquid, *if the process succeeds*, will not admit of the product being placed in the CLASS OF VARNISHES, destined for delicate painting; it is a kind of *saponaceous compound*, the use of which is not to be recommended in such cases."

In opposition to all this I have but a simple fact to produce: viz. It is many years since I dissolved copal by the process Instances of pictures varnished with the copal al-
luded to; and actually varnished several pictures with the solu-
tion so prepared: those pictures appeared to shine, or bear out, many years ago, and now in perfect and excel-
as artists call it, as well as if they had been covered with any
other varnish; the colour of the varnish has not changed in the
least, which most other varnishes would have done in the same
time; they have stood in a smoky house in London the whole
time, have been repeatedly washed with warm water, and
cleaned with spirit of turpentine; and notwithstanding all this,
they still preserve their glossy appearance undiminished. If a
solution which is proved by the experience of many years to
possess these properties in an eminent degree, does not deserve
to be admitted into the class of varnishes, it is not easy to tell
to what substance Mr. Tingry will grant that distinction.

Having said thus much for the fact, we may venture to pro-
ceed to the theory: When ammonia and copal were mentioned, The ammonia probably forms no part of the compound; but this is of no practical consequence.
the combination of both those substances with the spirit of tur-
pentine appeared to Mr. Tingry to be so much a matter of
course, that he took it for granted, and drew all the conclu-
sions from it without making a single experiment to prove the
reality of the fact, had he done so, even he, with the strong
prejudices he evidently has upon this subject, might have
doubted if the ammonia entered into the composition at all; to me
it appears, after attending carefully to the phenomena of the
solution, that it *does not*; but without determining positively
on the subject, I shall describe those phenomena, and leave
your readers to judge for themselves.

Particular description of the process for copal in sp. of turpentine by ammonia.

The success depends on the management of the circumstances.

Excellence of this varnish, &c.

Hint which produced the discovery.

The copal must be grossly powdered and put into a convenient glass vessel; the spirit of turpentine and ammonia must be well shaken together, and the mixture poured upon the copal; the vessel closed, and so much heat applied as shall, as speedily as possible, make the spirit boil so slowly that the bubbles may be counted as they rise to the surface; if this is skillfully performed, the solution is seen to take place in the following manner; the mixture has a milky appearance when first applied to the copal; the upper surface first becomes clear, the cloud gradually subsides, and attaches itself to the copal, which swells and becomes opaque; a portion of it in a dissolved state gradually exudes with the air bubbles, and diffuses itself in the spirit, and this continues till it has taken up as much as can be suspended in it; no more is then dissolved: It is on the skilful management of these circumstances that the success of the process depends; for, if less heat than the degree required be given, the solution stops; if more, the bits of copal instead of being opaque and adhering together at the bottom of the vessel, resume their transparency, and are agitated separately by boiling of the spirit; and when either of these accidents happened, I could never afterwards dissolve the same portion of copal either in the same or any fresh portion of spirits. If I might venture to explain these phenomena, I should say, that the ammonia gradually quits the spirit of turpentine, and unites with the copal, and reduces it to such a state, that with the assistance of the peculiar degree of heat that I have described, the spirit of turpentine is enabled to dissolve so much as will form a varnish; but, if either more or less heat is applied, the copal cannot be dissolved, and then the operation is abortive.

Whether this explanation be satisfactory or not is of small consequence, the existence of pictures in my possession, which were varnished with this solution many years ago, and still remain more perfect than they would have been if covered with any other varnish, is sufficient to prove that all Mr. Tingry has said on this subject has not the smallest foundation in truth.

I had examined all the books which I had access to, without getting any information respecting the solution of copal for the purpose of making varnish, till in Lewis's translation of Neuman's works I found the following, Vol. II. p. 27. "Solutions of copal have been greatly esteemed as varnishes, and the method of making the solution kept a secret in particular hands:

Junker

Junket informs us that it *readily succeeds, if spirit of sul-ammoniac, mixed with a due proportion of oil of spike or oil of turpentine*, be used as the menstruum." Upon this tint I proceeded till I had discovered the process I have described; but, though the varnish answered every purpose I could wish, the trouble of making it being great, and the failure of the process so frequent that I determined to seek for some more convenient method.

Having found that copal, when powdered and rubbed in the mortar with a small quantity of camphor, became soft, and in a few minutes, united in one coherent mass, I conjectured that by means of camphor, copal might be dissolved in alcohol, and upon trial found that it might be so dissolved, and rendered fit for the purposes of varnish; this I did, not by way of experiment, but in considerable quantities and at various times; so often, that I am authorised to say, that if the materials are good, and the process I have described in the Transactions of the Society for Encouragement of Arts be followed, there can be no difficulty in making a colourless spirit varnish from copal; and as a proof that this may be done, I send you a small quantity that I made some time ago, and which will enable you to judge whether it deserves to be "*placed in the class of varnishes.*"

But as it is improper, for many reasons, to varnish pictures with any substance dissolved in alcohol, I tried to dissolve the copal in spirit of turpentine by the intervention of camphor, and succeeded; not so easily indeed as in alcohol, but always with the certainty of success, when the precautions I have described were observed; this has been uniformly the case with myself for several years, as well as several artists who have made the trial; and it has now been proved by many years experience that copal dissolved, by means of camphor, in spirit of turpentine, is as colourless as the resin itself and perhaps as durable, since pictures that have been varnished many years with it have been preserved without the least change even in the colour of the varnish, which would not have been the case had any other substance been used.

As Mr. Tingry's experiments lead to very different conclusions, I am compelled to suppose, either that they were inaccurately made, or that the copal, camphor, alcohol, and * spirit

* Mr. Tingry himself, as he says, tried to dissolve copal in spirit of turpentine, and the process did not succeed: his notion that the spirit

Invention of camphor as a better medium than ammonia.

Sp. of turpentine preferable to alcohol.

This varnish is also excellent.

General
remarks.

spirit of turpentine in his country are very different from the substances which are sold by the same names in this.

My intention in communicating those papers to the Society was to assist those who are engaged in the practice of the arts, by making public a method of making a varnish that must be valuable to them: and I have been induced to trouble you with this, in hopes that, by printing it, you may prevent those false notions, which might be formed upon Mr. Tingry's experiments from gaining credit; to which you will allow me to add such hints and improvements as may prevent disappointment to those who undertake to prepare this varnish for themselves instead of depending upon those who make it for sale; this every artist should do if he resolves to have it perfect; for, though I do not mean to suppose that every maker of varnish will substitute inferior articles, yet it is certain that where the price of one article is higher, because it is really valuable, and difficult to make, many dealers will substitute something inferior that produces more profit to himself, without considering whether it may not be injurious to those who are to use it.

Narrative of an
artist who did
not succeed in
the process.

I had several times given some of my varnish to an artist, who liked it so well that he requested I would instruct him to make it himself, instead of making frequent applications to me for it: I procured him the necessary apparatus, and gave him a written account of the process, with such verbal instructions as I could add to it, and left him to procure his own materials and proceed to work by himself; he did so, and did not succeed in dissolving the copal; he tried a second and third time with no more success; he then brought his materials to me, requested that I would try if I could dissolve them; which experiment would determine whether his materials were bad, or whether there was any defect in his manner of attempting the process: this I resolved to undertake; using the same spirit

spirit I used had by chance obtained those powers for dissolving copal which he endeavours to give it by time, &c. is perfectly ridiculous. You and your English readers will know that in saying I did not succeed when I used spirit of turpentine from the colour shops, but always did when I got the spirit from Apothecaries Hall, I meant that at the latter place I always obtained the best article of the kind, instead of the inferior articles which are sold by the same name in other places, though totally unfit for this purpose.

and

and copal which he had thrice attempted in vain to dissolve; and at the same time subjecting another portion of my own materials to the same process in every particular: I succeeded in both instances: the only difference was that the solution which I produced from the materials which had been tried by my friend was not so viscid as that which was obtained from the fresh materials.

As the process which I have used for some time past is different from and better than that which I communicated to the Society, and has been published in their Transactions, I trust you will excuse me for describing it here; it is such an one as may be successfully used by any person upon a common fire, without any danger, and which any man of science, or who may chuse to adopt in a large way, may easily modify so as to answer his purpose.

Provide a strong vessel made of tin or other metal, it should be shaped like a wine bottle and capable of holding two quarts; it will be convenient to have a handle strongly rivetted to the neck; the neck should be long and have a cork fitted to the mouth, but a notch or small hole should be made in the cork, that, when the spirit is expanded by heat, a small portion may force its way through the hole, and thus prevent the vessel from bursting.

Dissolve half an ounce of camphor in a quart of spirit of turpentine, and put it into the vessel; take a piece of copal the size of a large walnut, reduce it to coarse powder or very small pieces; put them into the tin bottle, fasten the cork down with a wire, and set it as quick as possible upon a fire so brisk as to make the spirit boil almost immediately; then keep it boiling very gently for about an hour, when so much of the copal will be dissolved as will make a very good varnish; or, if the operation has been properly begun, but enough copal has not been dissolved, it may be again put on the fire, and by boiling it slowly for a longer time, it may be at last brought to the consistence desired.

The rationale of the operation I believe will be nearly as follows: copal and camphor have so strong an affinity for each other, that when separately powdered and then rubbed together, the copal absorbs the camphor, swells, softens, and becomes one coherent mass; nor when once united do they easily separate

Improved
process.

Safe and easy
method of mak-
ing copal var-
nish.

Explanation of
the process;

separate again *. When the camphor is dissolved in the spirit, the small bits of copal thrown into it and instantly made to boil, the motion of ebullition keeps the camphor suspended, moves the copal about in the spirits, the surface of each bit of copal is dissolved, and the whole of it softened and enlarged, and by continual boiling may be dissolved; but my friend did not proceed in this way; he reduced his copal to a very fine powder, thinking, perhaps, that would facilitate the solution; he then added the camphor, dissolved in the spirit of turpentine, but fearful of giving too much heat, set it by the side of his fire till it was warmed, then moved it into a warmer place, and at last made it boil; the consequence was that the camphor † left the spirit and united with the copal to form one solid coherent mass at the bottom of the vessel; the thickness of this mass prevented the heat from penetrating to the spirit so soon as it otherwise would have done; and, when at last it did boil, the camphor had entirely left it, and of course it was impossible that any solution should take place.

—and why Mr. S.'s friend failed.

Three times was the solution attempted in this manner, but when on a fourth trial by myself a sufficient quantity of camphor was added and the boiling heat immediately applied, the solution went on, but not so favourably as in my other attempt, because the copal was united into one mass at the bottom of the vessel, and no more could be exposed to the action of the solvent than the surface; but, in the other experiment the copal was broke into very small bits and the surface of each exposed to the full action of the solvent; of course a greater quantity was dissolved in the same space of time.

The instance of failure here mentioned might have been urged against the process.

Had this gentleman been satisfied with the experiment he tried by himself, he might have reported, from his own experience, that copal could not be dissolved by the process I had described; and yet in point of fact, nothing could have been more untrue: from this it may be fairly deduced that when persons who are unused to such experiments do undertake them they should be scrupulously exact in following the directions

* Six months ago I reduced some copal to this state; it has remained ever since exposed to the open air in a hot room, and still remains soft, the particles adhering to each other.

† When the spirit of turpentine was poured off from the copal, it was perfectly colourless, and by the smell did not in the least indicate the presence of camphor.

given

given to them, and on any apparent failure, resort to those whose experience may enable them to detect any fallacy there may be in the proceeding.

As it is well known that solution of copal is the most valuable of those varnishes which can be used for the finer works of art, and as it is proved that by the process I have described, these solutions may be easily made by any one in almost any situation, it only remains to mention some cautions which are necessary to ensure success.

First, That the spirit and camphor should be united before the copal is added.

Secondly, That the copal should be grossly powdered before it is added to the spirit, the whole should then be made to boil as *quickly as possible*, but not *violently*, and that ebullition should be kept up regularly till the solution be complete.

There seems to be no disadvantage in using too much camphor if it should so happen; but it is disadvantageous to put in too little copal; because a certain quantity of spirit will take up a given quantity of copal; and if more is added than will be dissolved, the superabundant copal will abstract more camphor from the liquid, and thus render the solution more tedious and difficult, and sometimes, perhaps, entirely prevent it.

The advantage that attends the use of glass vessels is that the solution may be seen all the time it is going on; the disadvantage is that persons who are not accustomed to such pursuits, may inadvertently give too much heat, and thus subject themselves to serious accidents; but by using vessels of metal with the precautions I have given, no accident can happen; the only disadvantage is that they cannot see what they are doing; this is of small consequence, as it may always be determined with sufficient accuracy by the ear whether the spirit boils, and to what degree; and as no accident can happen, without very gross neglect, from the use of them, they certainly should be preferred.

As I have written more than I at first intended, I shall conclude with a request that you will use your discretion in omitting any part or rejecting the whole of what I have written, if you think the publication will not answer any useful purpose. I send with it a small quantity of copal dissolved in alcohol, and

Remarks and
cautions.

Effects of an
overdose
of camphor or
of copal.

Glass vessels and
those of metal
compared.

Conclusion.

another

another in spirit of turpentine, which will enable you to satisfy yourself and your readers whether these solutions *deserve to be placed in the class of varnishes*, and remain

Your most obliged servant,

T. SHELDRAKE.

No. 5, Montague-Street,
Ruffel-Square.

IV.

Reply to Mr. DALTON, on the Constitution of Mixed Gases.
By Mr. JOHN GOUGH.

To Mr. NICHOLSON.

SIR,

Middlesex, Oct. 16, 1801.

Character of
Mr. Dalton's
reply.

I DID not attack Mr. Dalton's opinions openly, until he had invited me to do it; the attempt has been made on my part pursuant to his request; and he has replied, but in a manner which seems better calculated to amuse the superficial reader, than to convince the reasoner: For he treats the subject, sometimes with acrimony, and sometimes with ridicule; in the mean time his arguments are but few, and these appear to be negligently conducted. A bare inspection of his own letter will support the two first charges, and I will now enter upon the defence of the third.

Mr. Dalton begins by pronouncing the distinctions which I make betwixt a mechanical hypothesis and theory, to be nugatory; notwithstanding the causes of these distinctions are fully stated in my Strictures. Now controversy is of a nature which always obliges a man to point out the fallacy or futility of his opponent, when he can do it with success; but this has been neglected by Mr. D. in relation to my distinctions, and I leave him to draw the inference which results from these observations.

Mr. D.'s demonstration reviewed.

Mr. D. proceeds, in the next place, to demonstrate the fundamental proposition of his hypothesis; namely, the mutual penetrability of gases which do not attract nor repel each other. For this purpose he remarks, that oxygen repels oxygen, but not

not azote. This assumption is fairly allowed to be a postulate by Mr. Dalton; and he then proceeds to observe, that this being admitted, it follows, that if a measure of oxygen be put to one of azote, the oxygen finding it, *viz.* the azote, porous, enters its pores, and *vice versa*, &c. But here a second and an essential postulate is sily introduced into the syllogism, under the form of an inference; because no strict logician will venture to say that a body, A, must be porous of necessity, because it repels a body B; on the contrary he must conclude, that this mistake in his art destroys the demonstration, and does not retrieve the doctrine from the imputation of being an hypothesis. The simile of the philosopher, cottager, and sieve, Mr. D.'s simile may be calculated to promote ridicule, but it is badly chosen; incorrect. for it is impossible to demonstrate the proposed problem from Euclid's Elements, because this work confines its speculations to abstract figures and magnitudes. But a mathematician might demonstrate the same to a brother mathematician, from the elements of the mechanical philosophy; and he ought to have his demonstration ready upon demand, which is not the case of Mr. D. in the instance, of his fundamental proposition.

It is a matter of some surprise, that Mr. D. has drawn his demonstration from the mutual affections ascribed by him to oxygen and azote; seeing I had professedly attempted to shew the fallacy of his second and essential postulate, by ascribing the same affections to aqueous vapour and the permanent gases of the atmosphere. This unfortunate choice obliges my opponent to be content with a distant side-view of the arguments which have been brought against him; whereas a stricter observance of the path* chalked out for him, in my strictures, would have given him an opportunity to confront them, and defend his atmosphere of steam. The thing however has turned out, for some reason or other, as it is here represented; and Mr. D. arrives very briefly at the following singular conclusion; that though the demonstration of my proposition respecting air and vapour, be rigid, the previous data are not correctly assumed. Now this is the point which I endeavoured to establish in the proposition alluded to above; for his second postulate makes one of the incorrect data; in fact, they are all

The second postulate false in the instance of vapour and air.

* Phil. Journ. Vol. IX. Page 33.

his own except this: If a particle of vapour can pass freely through the air, a second can also succeed it at any given distance. My antagonist will certainly grant me this concession; because the salvation of his aqueous atmosphere obliges him not to suspect it. Should this indulgence be extended to my strictures, the demonstration in question must be re-examined with greater diligence by Mr. Dalton; for if it cannot be refuted, it will manifestly do away his atmosphere of free vapour, by proving the absurdity of his second postulate in the case of air and water; and the same observation may also be extended to all the permanent gases which are not absorbed by water; because no fluid of this description forces water to pass through its pores by pressing upon it.

The use of the
contemplative
analysis.

After all, if we may judge from Mr. Dalton's reply, he appears not to comprehend my arguments on this subject: they have however nothing of novelty in them; for the best reasoners in all ages have not hesitated to admit a hypothesis is for the sole purpose of discovering its worth, by comparing the consequences resulting from it with known facts. This kind of argumentation was in great esteem with the Greek mathematicians, and Pappus of Alexandria has described and recommended this species of logic under the name of the contemplative analysis.

Facts opposing
the hypothesis.

Mr. Dalton says, he is acquainted with no facts that contradict what I call his experimental probabilities. I can furnish him with one to which perhaps he is a stranger, and remind him of another, which he will not dispute: it will therefore be his business to reconcile them to the hypothesis of an aqueous atmosphere.

An experiment
by Mr. Kirwan.

First, Mr. Kirwan, in the course of his statical experiments on air, constantly found a given bulk of it to be lighter, *ceteris paribus*, when De Saussure's hygrometer was at 90° , than when the atmosphere was less humid. Now according to Mr. D.'s hypothesis, all the permanent gases, of this given bulk, had the same weight both in the dry and moist state of the air; because they were of the same density on account of the *ceteris paribus*: but the aqueous vapour was most abundant when the weight of the aggregate was least; that is, the weight of vapour diminishes, while its density increases. Will Mr. D. admit the truth of the preceding conclusion, to the detriment of his own doctrine; or will he refute the inference, by denying the fact?

Second, If the latter alternative should be preferred, the following experiment, with which Mr. D. is already acquainted, remains to be explained by the principles of his hypothesis, due attention being at the same time paid to the axioms of Dynamics. An experiment with a moist bottle.

A moist bottle, which I had found would contain 7794 grains of water of the temperature of 59° , was placed in water heated to 126° , having its mouth elevated about two inches above the surface. This aperture was covered by my hand; which was occasionally removed for an instant, that the warm air might escape. At the end of two minutes the bottle was inverted in cold water, the mouth of it being first secured in the manner described above; in which situation the temperature of it was again reduced to 59° . The air remaining in it after refrigeration, occupied 6172 of the 7794 parts constituting the capacity of the bottle. From this fact I infer, that 6172 parts of air of 59° , will occupy 7794 such parts after being raised to 126° in contact with moist glass. But 6172 parts of dry air of 59° , will only occupy 6992.66 parts, after being heated to 126° in a dry tube: Thus it appears that the presence of water augmented the bulk of the air which I used; how is this difficulty to be explained? We have no stopple of mercury in this experiment, by help of which Mr. D. explains the appearances of the manometer; consequently, as nothing was in the way to prevent the free egress of the vapour, it expanded itself in the bottle, and acted on the air contained in the same, just as it does in the atmosphere. But it increased the bulk of this air, which could not be effected on Mr. D.'s principles, otherwise than by distending the pores of it. Now his own tables of the force of vapour will shew this to be impracticable, if there be any truth in the axioms of Dynamics. On the contrary, nothing occurs in the two last experiments repugnant to chemical union; and as water makes a part of the atmosphere, it must be attached to air by the mediation of affinity.

I have now done something more than answer all Mr. Dalton's objections: the demonstration of his fundamental proposition has been shewn to be no demonstration at all; his second postulate has been proved to be false, by his own data, in the case of air and vapour; lastly, two facts have been advanced which are inexplicable by my opponent's principles:

ciples : After perusing these objections, can he blame me for rejecting his hypothesis. As for Mr. D.'s remarks on my theory of mixed gases, they require no answer ; because he professes not to understand it. I am willing, however, to submit the merits of it, as well as its claim to the title of a theory, to those mechanical philosophers who happen to comprehend it.

JOHN GOUGH.

V.

On the apparent Size of the horizontal Moon. In a Letter from Mr. EZEKIEL WALKER.

To Mr. NICHOLSON.

DEAR SIR,

Enlarged appearance of the horizontal moon.

WHEN the full moon is just rising above a clear horizon, she is an object that pleases every eye : But at the same time that she pleases the eye of the philosopher, she embarrasses his reason, to assign the cause of her apparent magnitude's being so much greater near the horizon, than at higher elevations.

Attempts to account for it.

When men in former ages began to reason on this phenomenon, they imagined that the angle subtended by the moon, was really increased, by a refraction of her rays, in passing through the vapours contained in the air, near the earth's surface. But when it became known that she subtended the least angle at the eye, when her apparent magnitude was greatest, philosophers said that the eye was imposed upon by the long series of objects interposed between the eye and the extremity of the sensible horizon. And after it was discovered that the same phenomenon was observed at sea, where no land objects could be seen, they blamed the clouds for deceiving them ; and when the clouds flew away, the spirit of inquiry flew after them, to seek for information in the apparent concavity of the sky.

Such were the erroneous opinions maintained by men of the greatest celebrity, both in ancient and in modern times. To mention any more of them would be useless labour : a single quotation from Dr. Smith's Optics will, I presume, be quite sufficient to show how little was known of this matter at the time when he wrote.

After

After the Professor had finished his celestial explanation of this phenomenon, he justly acknowledges that, at different times, the moon appears of very different magnitudes, even in the same horizon, and occasionally of an extraordinary large size, which he is not able to give a satisfactory explanation of. *Smith's Optics*, Vol. I. pa. 63, &c. *Remarks*, pa. 53.

It is really astonishing, that this phenomenon should have remained so long without an explanation founded on a better principle than that of mere opinion. That the dimensions of the pupil of the eye alter by the stimulus of light, has long been known; but I believe it still remains to be proved, that the picture of the moon formed upon the retina is not permanent, but varies as the dimensions of the pupil vary. The following experiments will, I hope, remove every doubt respecting this law of vision.

To imitate the eye upon a large scale, I took an achromatic lens of 1.6 inches in diameter, and 17 inches focal distance, to represent the crystalline humour. This I fixed upon a stand in a perpendicular direction, with a moveable white screen behind it for the retina, to receive the image of a lighted candle, which stood before it, at the distance of 40 inches. To imitate the pupil, I took three cards, and numbered them 1, 2, and 3. In No. 1, I made a circular hole half an inch in diameter, in No. 2 a hole of $\frac{1}{8}$ of an inch, and in No. 3, a hole of $\frac{3}{16}$ of an inch.

After I had moved the screen until the image of the candle appeared distinctly upon it, I found that the length of this luminous picture measured 1.82 inches.

When the card No. 1, was applied close to the lens, with the centre of the hole against the centre of the lens, the inverted image of the flame of the candle, upon the screen, measured 1.46 inches.

When the card No 2, was applied to the lens, in the same manner as No. 1, the image of the flame measured 1.3 inches.

And with No 3, the picture of the flame measured only 1.2 inches.

These experiments show us, in the clearest manner, the cause of that variation which obtains in the apparent magnitudes of objects.

That

Explanation offered upon the position that the images on the retina vary with the size of the pupil.

Experiment. A candle was placed before an achromatic lens, and three apertures were provided.

Focal image by the whole lens 1.82 inches long.

Half an inch aperture gave an image of 1.46 inch.

Three tenths gave 1.3 inch.

Two tenths gave 1.2 inch.

Application of the experiments to the moon and other objects,

That the pupil contracts as the quantity of light which falls upon the eye increases, is well known.

And that the magnitude of the picture of a luminous object upon the retina, decreases as the pupil contracts, is evident from the experiments.

Therefore, the magnitude of the picture upon the retina, decreases, as the quantity of light falling upon the eye increases.

Hence the apparent magnitude of the moon is greatest at the horizon, and decreases as she ascends; for the magnitude of her picture upon the retina is inversely as the quantity of light which she gives us.

and to the sun.

The sun appears larger at the horizon than at higher altitudes, for the same reason; and terrestrial objects seen through a mist appear larger than in a clear day, in consequence of the same operation of the eye.

I am, Dear Sir,

With much respect,

Your humble Servant,

E. WALKER.

Lynn, Oct. 13, 1804.

VI.

*Description of the Ship Economy, 200 Tons Measurement, built on the improved Construction of Mr. J. W. BOSWELL. **

THE plan adopted in the formation of this ship is that designed for large ships of 300 tons and upwards, and the third mentioned in my specification †.

Its external appearance is nearly the same as that of any other vessel of the size, and the outward planking done in the usual manner. It is the internal construction alone to which the patent relates, and that is as follows :

Description of a vessel framed in a new method.

The best general idea of it will be obtained by conceiving a vessel built with timbers, or ribs, much smaller than usual, with

* For which he has a patent.

† See Vol. II. Second Series, p. 81, of Repertory of Arts.

an internal framing, so contrived as to give every requisite support and strength both to them and the entire vessel, with the least timber, and of the cheapest form, and without any knee-timber.

Description of a vessel framed in a new method.

The floor-timbers are molded seven inches, and sided six: these, with four futtocks and two top timbers at each side, form what is called a frame of timbers. Those small timbers are laid down so that their terminations all fall out in fair lines, which are nearly the same as the ribband lines, when below the wales. Along those lines inside are laid fore and aft ribs, from stem to stern post, so as to support the extremity of every one of the small ribs in the ship. The fore and aft ribs are six in number at each side; one directly under the water ways, another at the level of the lower beams, and the other four placed nearly at equal distances between these last and the keelson: each pair uniting in a breast-hook at the stem.

The pieces of timber which form these fore and aft ribs are scarfed at their extremities with hook scarfs, and so placed that the scarfs fall out in fair vertical sections of the ship, where they are supported, and firmly bolted to transverse framings, contrived so as to unite the greatest strength with the least obstruction, and which are five in number in the whole ship.

Those transverse framings last mentioned must be considered as the great support of the vessel, and the foundations, as it were, on which all the other parts rest, as the beams of a wooden bridge are supported by the piers. Those transverse framings are each formed by one upper and one lower beam, two pair of futtocks, a floor timber, two pair of top timbers, and four bracing pieces; the whole connected into one firm framing, self-supported, independent of any other part.

The four bracing pieces form each framing into a set of triangular compartments: which triangular framing gives the greatest stability possible, as a triangular frame cannot be made to give in, or alter its figure, by any force which is not sufficient to tear its connecting parts through the timber of which it is composed; a property which no other figure possesses.

The leading principle is that triangular framings cannot vary.

These transverse framings (besides supporting the fore and aft ribs, and by them the small vertical timbers) tie and unite the vessel together across ship, so as to give much greater strength than hanging-knees, whose place they supply, at a much cheaper rate.

The

Framing of the deck.

The framing of the deck is also divided into triangular compartments, as specified in my patent, as to preclude the use of lodging-knees entirely; which compartments are formed by six pieces of timber, which proceed obliquely at each side, from the top of each beam to the fore and aft rib next adjoining, into which they are dovetailed and bolted; long carlings from beam to beam, at each side of the hatchways, with these pieces, support small ledges, on which the deck is laid in the usual manner.

The vessel in coming round from Southampton water sailed remarkably fast, and stayed and steered admirably well.

Advantages of this Method of framing Ships.

Advantages of the method:

1st. Timber of less than one fourth of the usual girth can be used, in this method, in constructing large vessels, for nearly four-fifths of their frames.

in price of timber;

This will be a direct saving in the difference of price of small timber and large for the quantity used; for large vessels this will be considerable, and, according to the present contract prices for naval timber, not less than from two to four pounds per load. Besides this, it is a great national benefit in another point; for, by this means, timber of half the number of years growth, or less, can be used for naval purposes; and thus forty or fifty years, or even less, be sufficient to produce timber fit for the navy, instead of the vast period of near a century, now necessary; by which the land will not only produce a double crop in the same time, fit for this purpose, but all danger be removed of there being a stoppage of building, for want of a supply of timber, at any future period; an event extremely probable to take place, from the increasing difficulty of getting the large kind used at present in the Royal Dock Yards.

use of shorter pieces, &c.;

2d. Much shorter timber may, in forming the futtocks, be used, without any danger of weakening the ship, on account of the great support given to them by the fore and aft ribs, and other internal framing, before described.

The advantage of this is, that it renders the compass timber for futtocks easier to be procured, and prevents any necessity of using any timber cut across the grain.

knees superseded;

3d. The use of knees of every kind is superseded by this mode of building, as the triangular framing of the decks gives

all the effect of lodging-knees, and that of the transverse frames more than supplies the support given by hanging-knees.

This would occasion a considerable saving in large vessels, on account of the great price of knee-timber fit for them; which, for that of 30 feet meeting, is near ten pound per load and for the smallest kind, taken at the dock-yard, not less than 8*l.* 15*s.*

4th. Plank of half the usual thickness may be used for lining; the great support given by the fore and aft ribs rendering any use of inside plank, to strengthen the vessel, needless, and confining its purpose merely to prevent ballast, or other matters, from getting between the timbers, so as to rest on the outside plank.

This will also cause a saving of consequence in large vessels; plank of all kinds, but particularly that of great thickness, being the next dearest article to knee-timber.

5th. It is probable a much less quantity of timber might be used with safety in this method, on account of the great strength produced for the timber used. 1st. By the triangular framing. 2d. By every timber having a solid support at each extremity. 3d. By the increase of thickness from in to out all along the fore and aft ribs, being very great in proportion to the timber used.

6th. It is probable, vessels built in this method will last many years longer before decay; because the use of small timber admits of a kind more spiny and durable than the large, which is often dotard, and never lasts so long; and also because this construction admits of a free circulation of air among the timbers, than which nothing is known to contribute so much to their preservation. It is moreover conceived, that the timbers being prevented from working by the solid support each has at its extremities, will cause the vessel to wear less, and at the same time render it safer, by diminishing the danger of starting planks, or otherwise causing bad leaks.

7th. The timber of considerable size used in this method is almost all nearly straight, or of very little curvature, on account of its running fore and aft.

This kind is much easier to procure than large compass timber.

8th. Short-top timber and coarse butts can be worked up to advantage, instead of being sold for less than half cost, or burned; as this kind will do sufficiently well for the number of short ledges in the deck frames, and to support the lining at the floor, which are wanted in this mode of building.

9th. Vessels built in this manner will not be so liable as others to hog, or have their backs broken, on account of the great strength length-ways, caused by the fore and aft ribs.

10th. Vessels so built will be drier; from the circulation of air before mentioned, and having the floor-lining detached from the timbers; which quality renders this construction particularly valuable for the ships used in the East and West India trade.

The advantages above recited relate to vessels entirely formed in this manner. It should be known also, that parts of this plan may be applied with profit. The mode of framing the decks, for instance, might be used to save lodging-knees in vessels built in other respects in the usual mode. Other parts of it might be applied to the strengthening old vessels, which, by this means, might be made to last many years, after they would otherwise have been unserviceable.

The principles of this method of building are capable of being extended still further than they are in the vessel here described: the triangular framing may be even adopted to the construction of fore and aft ribs, so that these could also be constructed of small timber, if required. Thus, by this means, the former barrier to the increase of size in ships is removed, as it no longer now depends on the size of timber; and ships of any dimensions required may be formed, of any strength requisite, of small timber.

J. W. BOSWELL.

VII.

Concluding Remarks on the Computation of Tables of Squares and Cubes. In a Letter from E. O.

To Mr. NICHOLSON.

SIR,

I AM sorry to trouble you again on a subject which I fear On the computation of squares and cubes. can afford but little interest to the generality of your readers; but I must request the admission of a few remarks upon the letter which you published in your last number from H. G.

Your correspondent is certainly mistaken when he asserts that our methods of calculating squares are "precisely the same." Both, indeed, are derived from the principles of the binomial theorem; but I conceive that there is a material difference in our manners of applying those principles to practice. As we find the square of the given number by adding a certain quantity to the known square of the number next below it, it follows, that in both methods the quantity so added must be the same; but there is a considerable difference in the operations by which we find this number: I do it by the repeated addition of the small number 2, whereas H. G. finds the same quantity by the repeated doubling of a number, which is constantly changing, and may possibly consist of many figures. To shew the difference more clearly, I will calculate several squares according to the direction given in P. 150, Vol. VIII. and I will take for my example the same which I have taken in P. 6, Vol. IX. that your readers may the more easily form the comparison for themselves.

28261	28262
2	2
<hr style="width: 100%;"/>	<hr style="width: 100%;"/>
56522 = 2.28261	56524 = 2.28262
798684121 = $\overline{28261}^2$	798740644 = $\overline{28262}^2$
1	1
<hr style="width: 100%;"/>	<hr style="width: 100%;"/>
798740644 = $\overline{28262}^2$	798797169 = $\overline{28263}^2$

On the compu-
tation of squares
and cubes.

28263		28264	
2		2	
<hr/>		<hr/>	
56526	= 2.28263	56528	= 2.28264
798797169	= 28263 ²	798853696	= 28264 ²
1		1	
<hr/>		<hr/>	
798853696	= 28264 ²	798910225	= 28265 ²

Perhaps it may be objected to my statement, that the calculator would be aware that $28264.2 = 28263.2 + 2 = 28262.2 + 4$, &c. or that he must soon discover this law of continuation; and therefore, that he would not proceed in every instance to the actual multiplication of the root by 2. Such an objection, however, will avail nothing against my argument, for in that case he will no longer work by the rule given by H. G. in P. 150, Vol. VIII.; but if he proceeds to find the difference by addition, he must virtually pursue the method which I recommend: the only difference will be, that the practice of calculation will have suggested to him an improvement which he might have originally derived from first principles.

I shall not enter into the question, whether addition is, or is not, a simpler process than subtraction. The present question may be decided independently of it. For granting that there is no reason to prefer one to the other, yet the two methods by which we find the first differences of the cubes, might be fairly put to issue upon the number of figures used in each. To find them according to the method recommended by H. G. in p. 150, Vol. VIII. we must subtract the whole of one cube from the whole of the other; whereas I find them by the constant addition of the second differences, which must necessarily be much smaller numbers. Thus to find the first difference between the cubes of 26561 and 26560:

<i>According to H. G.</i>	<i>According to E. O.</i>
18738432796481	2116221121
— 18736316416000	+ 159360
<hr/>	<hr/>
2116380181	2116380181

It is stated, indeed, by H. G. that he avoids the continual repetition of first differences. By this we must not understand that

that his method makes it unnecessary to calculate them; for On the computation of squares and cubes. by mere comparison of p. 9 with p. 125, it will be seen that we both use the same numbers. In this instance, as in that of the squares, the real difference between the two methods consists in the manner of finding these additional quantities. H. G. must therefore mean, that he avoids the necessity of calculating the table given in p. 8. Now in this I do not think that any advantage is gained. For if the first differences are to be found separately, I have shewn that they can be more easily found by the method which I have recommended; and much expedition will be obtained by making a separate table of them, which is impossible if we work according to the directions of H. G. I may, with some reason, complain, that H. G. has not made his example a fair parallel to mine; for I have set down every figure which need be used, while he has declined printing any of the operations whereby he determined his first differences. It is possible, indeed, from long practice in arithmetical computations, that he may be able to cast his eye from one cube to the other, and so find the first differences, without copying them out on the side of his paper for subtraction. I shall not dwell upon the danger of mistake in such a process: the quickest and most accurate eye could hardly avoid frequent errors, and the common arithmetician will find it attended by considerable difficulty. But granting that all this difficulty could be completely conquered, still it would give no superiority to the method recommended by H. G.; for it might be applied to mine with equal, if not greater ease and advantage. Thus, in the example given in p. 125; if F were computed by the addition of C and D, instead of the subtraction of A from E; if we consider $I = F + G$ instead of $= H - E$, $M = I + K$ instead of $= L - H$, &c. we should have the differences computed, according to my rules. We may remark likewise, in this shorter method of calculating, that, according to the arrangement given by H. C. himself in p. 129, the eye must necessarily pass over two intermediate lines of figures in subtracting one cube from the other, whereas the two rows of figures which I would have added to one another, may always be placed contiguous. This certainly is an advantage; but at the same time I only mention it to shew the grounds

On the comparison of squares and cubes.

grounds on which I prefer my method to that of H. G.; for I still consider the plan which I first laid down as the easiest and surest. . .

I have said, that if the differences are considered in p. 125 as found by addition instead of subtraction, the example would suit with my method, because the second differences are still additional, and will be found, upon comparison, to be added in exactly the same order as in pp. 8 and 9. The only variety is by making two tables: I add the sum of the two numbers which are separately arranged under each cube. I have only to add on this part of the subject, that H. G. has adopted my method in some measure; for he says that $K = G + 6$, as well as $= 6d$. It is true (as I remarked with respect to the squares, that this equality would most probably occur to the calculator; but at the same time we must recollect, that it is different from the rule laid down in p. 150, Vol. VIII.

I shall not add any thing to what I have stated in p. 5, with respect to the mistakes to which either method is liable; but I shall conclude by assuring you, that it is not my intention to trouble you any more on the present subject. I thought it might be useful to publish the letter which I first sent you, and having done so, it became necessary to remove any obscurities in it, and to answer any mistaken objections which might be raised against it: but this having once been done, there can be no occasion for further controversy; and I have only to thank you for the indulgence which you have granted me.

I am, Sir,

Your obliged humble servant,

E. O.

P. S. I take this opportunity to beg you will mention two or three press errors which occur in my former letter.

P. 9, line 20, delete the Italic *e*.

P. 10, line 12, $\overline{x + 1}^3$, read in both instances, $\overline{x + 1}^2$

P. 12, line 16, 29 read 27.

VIII.

Letter from C. WILKINSON, Esq. on Galvanism and Electricity.

Dublin, Oct. 8, 1804.

To Mr. NICHOLSON.

SIR,

WHEN at Liverpool a few days since, at the Athenæum I found a paper of Mr. Thicknesse of Wigan in your valuable Observations on galvanism, &c. Journal, and which has induced me to trouble you with the following observation. Mr. T. ingeniously conjectures that the galvanic phenomena depend more upon the decomposition of the water employed than as to any chemical change effected on the metals. Mr. T. observes that two metals are requisite to the production of galvanic phenomena: this is no ways the case, it is well known that a single metal suffices, or even brain and muscle, according to the experiment of La Grave, or nerve and muscle according to Aldini. Mr. T. further observes that hydrogen mixed with copper always renders it brittle. I should wish to know this gentleman's authority for such an assertion: he also sets out too hypothetically as to electricity being a modification of caloric. It has always appeared to me that the galvanic phenomena entirely depend upon the disengagement of electricity from the metal undergoing a chemical change.

Galvani has ascertained that gold, silver, copper, iron, tin, lead and zinc, constitute the series of metallic bodies; that when two metals the most remote in the series are united, the most powerful galvanic combination is formed; thus gold and zinc, silver and zinc, copper and zinc, &c. and lastly, lead and zinc, form the weakest galvanic combination. The disposition to oxidation is in the inverse order, thus zinc will become oxidated even by exposure to the air, whilst silver and gold undergo this change with the greatest difficulty.

Hence a metal which is oxidated with the greatest difficulty combined with a metal which oxidates with the greatest facility, form the most powerful galvanic combination. The production of galvanic phenomena is always proportionate to the degree of oxidation.

ALL

Observations on
galvanism, &c.

All substances which are conductors of electricity, if they undergo any change as to their conducting power, so as to become diminished as conductors, always in the change lose a portion of their natural electricity. Thus metals which are the best conductors, when oxidated, become non-conductors in this change, the combined electricity is lost.

If a plate of zinc is universally immersed in a fluid which will produce chemical changes upon it, no galvanic phenomenon will be produced because the metal in every assignable point, being equally acted on, the contrary states of electricity cannot then be produced.

If only one side be acted on, every assignable point on that side undergoing a chemical change, by which it is disposed to part with its combined electricity, there will be a general tendency of the electricity in the substance of the metal towards the surface acted on; the same as if in a vessel filled with water, a small aperture be formed, there will be a general tendency or current of the water towards the aperture, so of electricity.

Dr. Wollaston has proved by very ingenious experiments, that when two dissimilar metals are placed in a fluid which will act upon one of the metals and not upon the other, when the two metals are put into contact in the same fluid, that thus chemical changes are effected on both; thus that gold and silver thus arranged, will be acted on by the nitric acid.—When two metals are placed in a fluid which will act upon either of them separately, if the two metals are preserved in a state of separation while in the same fluid, chemical changes will only be effected upon one. Thus zinc and copper placed in nitrous acid and water, only the zinc will be acted on.

These principles point out the necessity in our galvanic troughs to have our cells perfectly insulated, and that there be no communication between the respective plates.

Mr. T. has by mistake observed that the sensation is in proportion to the surface acted on; the experiments of the French philosophers proved that the action of galvanism on animal substances is in the ratio of the number of plates employed, and not the surfaces exposed.

In all my experiments in town with my own electric battery I never succeeded in giving the slightest charge with my galvanic apparatus. In some conversation I had with you, Mr. Editor, you conjectured that an extensive series of small jars would

would be the best arrangement. When at Liverpool, I met with such at the house of Mr. Dalton, a very ingenious lecturer on electricity. The galvanic battery I employed consisted of 200 eight-inch plates, which had fused at the time near five feet of steel wire; this communicated a very slight intensity to the electric battery, containing forty feet coated surface, just sufficient to convulse a frog, and although the electrical battery was in the best state of preparation, we could not succeed in producing any intensity as even to affect the tongue. At another period, I shall do myself the pleasure of transmitting to you an account of some ingenious experiments of Mr. Dalton, on electricity, he finds a cylinder, when exhausted of air, or when one atmosphere is condensed into it, will not then be capable of excitation.

I am, Sir,

Your's sincerely,

C. WILKINSON.

No. 19, Soho-Square.

IX.

*Method of preventing Accidents to Horses and Carriages, in going down Hills, by a Gripe or Clasp acting on the Naves of the Wheels of the Carriage. By Mr. W. BOWLER.**

SIR,

THE invention I have now sent you is, I think, likely to be of great use, and I therefore offer a model thereof for your inspection. In the first case, this cart may be stopped in an instant, in going down the steepest hill with a load, without stopping the horses, by the carter only pressing his hand upon a lever. This plan would likewise be highly advantageous in case of a horse taking fright, as the carriage may be instantly stopped by the brace clasping the wheels. It may also, with little alteration, answer for a broad-wheel waggon with a heavy load. By the present method, a coachman, whilst sit-

Apparatus to lock the wheels of carriages.

* Transactions of the Society of Arts. A bounty of ten guineas was given to the inventor, and the Society have a model.

VOL. IX.—NOVEMBER, 1804.

N

ting

Apparatus to
lock the wheels
of carriages.

ting on his box, may confine or release the wheels of his carriage at pleasure, and prevent accidents in descending hills, or in managing restive horses.

I am, Sir,

Your humble Servant,

WILLIAM BOWLER.

Finbury-Street, May 13, 1803.

To Mr. CHARLES TAYLOR, *Sec.*

Reference to the Engraving of Mr. William Bowler's Gripe for the Wheels of Carts and Carriages, Plate X.

A B. The extent of the body of the cart. C D. The two shafts. E. The front ladder of the cart, part of which projects over the shafts, and is supported on them: the other part is fastened to the upright piece F. G. The handle of the lever, sliding betwixt two uprights, and moveable on an iron bar and pivot at H, in the upright piece F. By pressing down this handle, the moveable joints I K raise upwards the lower part of another lever L, so as to press against the lower part of the nave of the wheel, and at the same time the moveable joints press down the lever O, and cause this lever to act against the upper part of the nave of the wheel; thus compressing the nave of the wheel betwixt a double brace, and either retarding wholly the motion of the wheel, or allowing it to move a little, as may be thought requisite. The circle of the nearer wheel is shown by the dotted lines P; but this wheel is not added, as it would prevent this gripe from being clearly seen. The handle of the lever G produces at the same time a similar effect on the further wheel R, by means of the bar of iron which crosses the cart at H, and acts in the same manner on similar double joints at the other side of the cart, as may be seen at S, where the further double gripe is shown pressing on the nave of the further wheel. T. A small catch, which, by falling into the notches or teeth of the lever, when it is pressed down, holds both the gripes in one certain position.

When the wheels are to be set at liberty, the catch T is removed out of the teeth of the lever; then the lever G is raised above the hole V, and kept from sliding down by an iron pin attached to the chain U, being put underneath the lever, through the hole V.

The

The iron-work is not any inconvenience in the loading or unloading of the cart, nor occupies any room within it; and, what is of material consequence in new inventions, it may be managed by the carter with much less time and trouble than the common hook and drag chain. It is also much more secure in its action, as it binds on both wheels at once, and gives a uniform steady draught for the horses, which prevents them from falling down.

X.

Observations and Experiments to elucidate the Operation of the Galvanic Power. By Mr. CHARLES SYLVESTER. In a Letter from the Author.

To the EDITOR of the PHILOSOPHIC JOURNAL.

SIR,

I HAVE for some time read and admired your valuable publication, but have not before this time ventured a communication on any subject. If you think the following thoughts and experiments worthy the attention of your readers, I shall feel myself honoured if you give them a place in your Journal.

I remain, Sir,

Your obedient servant,

CHARLES SYLVESTER.

Sheffield, Oct. 16, 1804.

The decomposition of water, or at least, the presence of oxygen and hydrogen, is observed in all the modes by which the galvanic energy is excited. I observe that some of your correspondents are inclined to doubt the truth of water being a compound body, from observing in its decomposition that the oxygen and hydrogen are given out at so great a distance from each other. Though these philosophers have more simply accounted for some of the phenomena by making water a simple substance, their assumed data, that positive and negative electricity are two distinct bodies, is by far more gratuitous than the position, that water is a compound. Besides this hypothesis will only account for a few of the facts, while it abounds with contradiction when applied to the rest of the phenomena of galvanism.

Whether water
be decomposed
in the galvanic
process;
—or pos. and
neg. electricity
be distinct
bodies.

They tell us of two kinds of electricity coming from the galvanic apparatus one from the zinc end, the other from the copper end: the former being positive, the latter negative.

The latter less probable.

Two distinct fluids moving in contrary directions could not possibly exist in the galvanic trough; by reason of that fluid proceeding from the zinc meeting that coming from the copper in each of the cells, and consequently establishing a constant equilibrium, without producing any effect. When the decomposition of water is only taking place in one vessel (admitting the above objection to be of no weight) the phenomena admit of an easy explanation by this hypothesis; but when two vessels are used connected by a wire arch, it cannot in any shape be adequate; for as we have in each vessel both oxygen and hydrogen gases, there must also be both positive and negative electricity; these two contrary fluids would of course have to meet each other in the connecting arch, and according to another part of this theory form sensible heat*.

The oxidation contradicts the hypothesis of two electric bodies.

The positive wire, if a base metal is always oxidated; this effect is known to be facilitated by the electrical agency. According to the principles in question, it would be retarded by the power of affinity existing between water and positive electricity. When the wires of a galvanic apparatus are brought into a metallic solution, no hydrogen is given out at the negative wire, but the metallic oxide is reduced. Now if metallic oxides be composed of water and the metal, the negative electricity would combine with the water, and hydrogen gas would be evolved—the very reverse of the fact.

My reason for being thus particular in the examination of the hypothesis of Mr. Richter and Dr. Gibbs, is to prepare the mind of the reader for some experiments which I think will contribute to the firmness of the hypothesis, which supposes water to be a compound, and that the hydrogen is carried from the positive to the negative wire by the electricity.

Variation of Dr. Ash's experiment. When a long copper wire was connected at the end with zinc (under weak sulph. acid) hydrogen was earlier given out

The well-known experiment of Dr. Ash (with the plate of zinc and silver in a dilute sulphuric acid) I had occasion to vary in the following manner. I took a copper wire about eighteen inches long, and bent it in the middle so as to form

* They are of opinion that these two fluids combined form caloric. They also hold with Dr. Priestly that what are now termed metallic oxides are the respective metals combined with water.

two parallel legs, about two inches distant from each other. ^{at the nearer parts of the (bended) wire :} This I laid in an earthen dish filled with dilute muriatic acid. I then brought a piece of zinc in contact with one of the ends: bubbles of hidrogen were immediately given out by that part of the copper. After bubbles had appeared for about two inches down this leg, I observed bubbles on the end of the other leg,—they now proceeded down the two legs till at last they appeared on the part where the wire was bended. I afterwards laid a piece of zinc at one end of the same dish and a piece of gold at the other. I prepared a metallic arch with two pieces of wire, one of gold, the other zinc, folded together. The zinc end I connected with the piece of zinc in the dish, and the gold end with the gold. After the contact had been made about ten seconds, bubbles appeared on the piece of gold. The distance was about twelve inches. I now placed the pieces of gold and zinc at half the distance, and made the communication with the same arch. The bubbles now appeared upon the gold in about half the before-mentioned time. Upon bringing the pieces still nearer together, the gold gave out gas almost instantly, but yet in all the three instances I had time to observe that the bubbles always appeared first on that side the gold next the zinc. —and so like-wire in a compound conductor between gold and zinc.

I think from the above experiments that the disengagement of hidrogen from the gold does not depend upon its negative state, for this quality must have been produced on the gold in every one of the three instances in the same time, from the communication being always made by an arch of the same length. On the contrary, it proves that the electricity disengaged from the zinc is communicated to the gold through the intervening liquid. If we observe the length of time taken up in its passage through this medium, we shall see that it does not agree with the laws of electricity. Is it not therefore probable that when the oxygen of the water combines with the metal, the hidrogen combines with the electricity of the metal, and a compound of this kind observes those laws consistent with the phenomena. Whence it is inferred that the disengagement of hidrogen does not depend on a neg. state; but that the electricity is communicated through the interposed liquid.

In each of the cells of the galvanic trough a quantity of this compound is diffused through the liquid. The moment a communication is made between the two ends, the electricity enters each of the copper-plates, leaving the hidrogen in bubbles

on

Probability that the hidrogen is in combination with electricity.

Objection to the
apparat^{us} de-
scribed in our
Vol. VIII. p. 3.
on the couronne
of Volta.

How the elec-
tricity increases
the hydrogen in
the exterior
cells.

on the surface *. As this electrated hydrogen (if I may be allowed the expression) does not move through the liquid with the facility of the electricity itself, the necessity of a surface of copper equal to that of the zinc is obvious. It is plain that only a proportionate quantity will be carried through the whole series. This fact I have ascertained in attempting to render the apparatus proposed by Mr. Wilkinson and yourself useful for all experiments. The shock is even less when the surface of copper is less than a square inch.

The quantity of electrated hydrogen in each cell increases from the copper end to that of the zinc in an arithmetical progression. The electricity only exists in its simple form while it is passing from the copper surface through the two metals to the zinc surface. It there combines with another portion of hydrogen, which it leaves at the next copper surface, and so on accumulating in quantity the longer the series it has to pass through. In the decomposition of water by the galvanic trough the vessels in which this is performed may be considered as one cell in the series. The electricity seizing the hydrogen at the positive wire and giving it out at the negative.

XI.

Memoir on the Origin of Wax †. By FRANCOIS HUBER,
Member of the Society of Natural Philosophy and Natural
History of Geneva.

IT has been thought strange that the word wax should seldom occur in a book which treats of bees alone: but nevertheless as in the course of my observations, I had not attended to the products of their industry, I could only have repeated what had been said by Swammardam and Reaumur, and that did not seem to me to be necessary.

Mechanical
operations of
the bees.

I knew that these insects collected abundantly upon the anthers of flowers, that they are acquainted with the method of

* This fact is very obvious when the copper surface is very clean. The hydrogen under contrary circumstances is employed to reduce the oxide.

† From *Journal de Physique*, &c. Pluviose, An. XII.

opening

opening them, of gathering their dust, keeping it in the cavities of their hind legs, and carrying it to their hives.

It had been observed that the particles of this dust swells in water, and that, when one of them bursts, an oily liquor runs out, which floats on its surface, but did not mix with it; from these experiments, repeated on the dust of a great number of flowers, it was concluded that they contain the principles of wax, but it was admitted that these must undergo a peculiar elaboration in the body of the bee, since, according to the experiments of Reaumur, a flexible wax could not be made from the dust of the antheræ.

It will be seen from several passages in my work that I had adopted this opinion: a single observation of Burnens* changed all my ideas. The true origin of wax might have been sooner known, had there been any suspicion that it was not already discovered. I shall now state how I was led to doubt, and what I have done to verify my new conjectures.

I was in Switzerland in 1793; the farmer of the estate on which I resided had many bees, and the greater part of his hives having been stocked in former years, the combs with which they were filled reached to the stands, consequently there was no room to construct new ones. We remarked, however, that the working bees carried in a considerable quantity of this fecundating powder.

There was also in the same apiary some swarms of that year, the hives having only been stocked a day or two; in some of them the combs were only began, in others they were larger, but in all of them there were vacancies to fill up, and much work to do. We observed with astonishment that the bees of these swarms did not carry in the pollen, and that, nevertheless, they worked with activity in the construction of new combs, and in lengthening those already commenced. Where, therefore, did they procure materials for their edifices? After these observations, we suspected that it was not from the dust of the stamina, and that they had a very different use for it than that for which it was believed to be intended. We however found that it was not impossible to explain these extraordinary facts, without abandoning the hypothesis of Reau-

* The name of M. Huber's secretary; which deserves to be known to the cultivators of natural history.

mur, by supposing that the bees of the old hives stored up so much pollen in their combs for their future wants, while those of the new swarms did not carry it outwardly on their legs, in the infancy of their establishment, because they had no cells in which they could deposit it: it might be sufficient to enable them to construct their combs, if they were at liberty to fly to the flowers, procure their pollen, and return to their hives after having filled their stomachs, where it must be elaborated and converted into perfect wax. It was to obviate these doubts that I undertook the following experiments.

FIRST EXPERIMENT.

On Bees in Confinement with Honey alone for their Nourishment.

Exp. I.
Is pollen necessary to the production of wax?

Must bees eat pollen to be in a state to produce wax? This was the first question which I thought it necessary to investigate: the method of trying the experiment was obvious; it was only required to keep the bees within their hives, and thus prevent them from collecting or eating the fecundating powder.

On the 24th of May, Burnens lodged a swarm in a straw hive, with as much honey and water as was necessary for their consumption, and he closed the doors so that the bees could not get out and the air be at the same time renewed.

Wax from
honey alone.

At first the bees were very uneasy, but became calm on removing the hive to a cool dark place: their captivity lasted five days; they were permitted to come out in a room, the windows of which were shut: we then examined the hive more conveniently. We first noticed that there was no honey left in the vessel which had been filled with it, with the sole intention of feeding the confined bees; and were more astonished to find five combs of the most beautiful wax, suspended from the roof of the hive; they were perfectly white, and very brittle. This result was very remarkable; however, before forming a conclusion from it, that the honey with which these bees were fed had enabled them to produce the wax, it was necessary to enquire whether it could not also be explained in another manner.

The bees which I had employed had doubtless collected the dust while they were at liberty.

They might have done so the evening before, or on the very day of their confinement, and might have enough in their stomachs, and in the cavities of their legs, to extract from it all the wax which we had found in their hive.

But

But if it was true that it had been obtained from the fecundating powder previously obtained, this source was not inexhaustible, and the bees being unable to procure any more, they would soon cease to construct combs, and fall into the most complete inaction. It was necessary therefore to continue the same trial to render it decisive.

The 28th, Burnens returned this swarm into its hive, after having taken out all the combs; he shut them up as before with a fresh supply of honey.

This experiment was not long, for on the evening of the second day we perceived the prisoners working with new wax. The next day the hive was inspected, and we found five combs, as heavy and as regular as those made during their first captivity.

We afterwards repeated this experiment five times successively, with the same bees, and the same precautions; we always found that the honey had disappeared, and that new wax was produced. This result was so invariable during this long seclusion that we could no longer doubt that the honey alone had supplied them with all the elements of their wax, without the assistance of the fecundating dust.

SECOND EXPERIMENT.

On a Hive from which Honey was excluded, and in which only Pollen and Fruits for the Nourishment of the Bees were left.

I thought it would not be useless to make the inverse of the Exp. II: preceding experiments: it would show me whether the pollen could not supply the want of honey when the bees were deprived of it, and enable them to produce wax.

I therefore enclosed a swarm in a bell-glass, in which had been placed a comb whose cells contained only pollen, and the sole nourishment of the bees was fruit.

These bees did not make wax, nor did they form a single cell during eight days, which was the time of their captivity.

I was going to repeat this experiment, when Burnens re-Bees fed on marked that the free bees were, in some measure, in the same pollen and fruits state as those we had confined; there being no honey at that wax. do not produce time in the flowers, they found only pollen, and did not work in wax.

I may

I may perhaps be asked how I was satisfied of this, to which I answer, bees-wax is white at first, the cells soon become yellow, and in time, this colour grows browner, and in older hives the combs have acquired a blackish tinge. It is therefore very easy to distinguish the new cells from those which have been some time formed, and consequently to know whether the bees are really making combs, or whether that work is suspended; it is sufficient to raise the hives, and to notice the lower edges of the combs.

Wax-making
bees.

Nursing bees.

The odour exhaled by the hives, and the shape of the bees, are indications by which it may always be known whether there is honey in the flowers; if they are combined, there can be no further doubt, and, particularly, if a great number of bees return to the hive, which are remarkable for the bulk and the form of their bellies. Those which are filled with honey have the abdomen cylindrical; the name of wax-making bees belongs to them exclusively: the bellies of the labouring bees which have other functions, always preserve their ovoid form, and their volume is never sensibly augmented; the name of nursing bees is proper for these.

Operations of
the bees during
an intemperate
spring,

The farmers of the neighbouring villages kept their bees in baskets, or in cases of different forms; and I was able to visit a very great number without going to any great distance from my habitation.

In 1793, an intemperate spring had retarded the separation of the swarms; there had not been any in the country before the 24th of May; but towards the middle of June there were several in the vicinity of my residence. At that time the fields were covered with flowers, the bees collected much honey, and the new swarms worked at the wax with vigour.

On the 18th, Burnens visited sixty-five hives; at the entrances of all of them he observed wax-making bees. Those which returned to old hives, not having to construct cells, deposited their honey in the combs, or distributed it among their companions; those belonging to the swarms converted their honey into wax, and hastened to construct combs for the reception of their young bees.

It was showery on the 19th: the bees went abroad but brought home only pollen. The weather was cold and rainy until the 27th. We were desirous of knowing if this had prevented their working. On the 28th, all the hives were lifted:

Burnens

Burners found that the work had been stopped; the combs which he had measured on the 19th, were not at all increased, and were of a citron-yellow, nor was there a single white cell in any of these hives.

On the 1st of July the chefnuts and limes were in blossom, —and a dry summer. the thermômeter indicated the 20th degree; the wax-making bees re-appeared, they carried away great quantities of honey, which, as we had before observed, was employed in augmenting the provisions of the old hives, and in enabling the young swarms to construct new combs.

The greatest activity was observable among them: the gathering of honey, and the production of wax continued until the middle of this month.

July 16th, the heat remained the same: the field flowers, as well as those of the chefnuts and limes, were completely withered; they yielded no more honey; their pollen alone attracted the working bees, and they collected it abundantly, but there was not any wax produced: the combs were not lengthened; those of the young swarms did not fill more than two thirds of their hives.

August 9th. It had not rained for six weeks, the heat was very powerful, nor was there any dew to allay it during the night: the black wheat which had been in flower for some days, did not offer any honey to the bees; they found only pollen.

On the 10th, it rained for several hours; next day the black wheat had the odour of honey; in fact it might be seen glittering in their expanded flowers. The bees found enough to feed them, but too little to induce them to work at new wax.

On the 14th, the drought re-commenced, and lasted to the end of the month: no more honey appeared upon the flowers, and when we visited the sixty-five hives for the last time, we found, 1st, that the bees had not produced any wax after the middle of July; 2d, that they had stored up a great quantity of pollen; 3d, that the supply of honey was much lessened in the old hives, and that hardly any remained in the new swarms, that which they had collected in the spring having been employed in the preparation of wax; the pollen, therefore, has not this property, and no further doubt remained on this head.

This year had not been stormy, and I have since ascertained, by a great number of observations, that electricity is singularly favourable to the labour of bees.

favourable to the secretion of honey by the flowers: the bees never collect it in greater abundance, nor is the preparation of wax ever more active than when the wind is in the south, the air humid and warm, and a storm gathering.

Heat too long continued, and the drought which is the consequence of it, cold rains, and principally a north wind, suspend it entirely.

THIRD EXPERIMENT.

On the Use which the Bees make of the fecundating Powder.

Exp. III.

In the second experiment the bees did not touch the pollen which I had placed within their reach, and, as its quantity was not sensibly diminished during this trial, I was induced to believe that it was not an aliment proper for them.

What is the use of pollen?

I also knew that the new swarms were liable to perish from hunger in the middle of summer, and even when the country was covered with flowers, if a particular temperature, which is too uncommon in our climate, did not favour the secretion of honey in their nectaria. What, therefore, is the use of the pollen which they collect with such avidity during eight months of the year, and of which they lay up such abundance? *

This question remained to be investigated.

I had a hive, in divisions, the queen of which was barren; its combs did not contain any pollen, but they had much honey: the two narrowest sides of this hive were formed of panes of glass, through which the surfaces of the exterior combs might be seen, and the conduct of the bees observed.

The queen, bee taken away.

I took away the queen on the 16th of July, but to console the working bees I removed the first and the twelfth combs, in which there was not any thing to interest them, and I supplied their places with two combs, the cells of which were filled with eggs, and worms of all ages. I carefully cut away all the cells in which pollen could be perceived, and shut up the

* Reaumur was of opinion that the bees of a well-stocked hive might collect at least a hundred pounds of this substance in the course of a year; but, having remarked that the weight of wax, fabricated in the same time, did not exceed two pounds, he concluded "that the bees extract only a very small portion of the true wax from this native wax, that the greatest part of it is required for their nourishment, and, that the rest is discharged from their bodies in the form of excrement."

hive

hive with a grating. My intention will be guessed: I wished to know whether these insects could support their young without this fecundating powder.

The next day nothing extraordinary occurred; the bees sat on their eggs and seemed to nurse them.

On the 18th, after sun-set, a great noise was heard in the hive. Anxious to see what occasioned it, we opened the shutters, and observed that all was in confusion: the incubation was stopped; the bees ran over the combs in disorder; we saw thousands precipitate themselves on the stand, those which were nearest to the mouth eagerly gnawed the grating; their intention was no longer doubtful, they wished to get out of their confinement.

I was fearful of destroying them by continuing to prevent them from yielding to their instinct, they were therefore set at liberty: the whole swarm came out, but the hour was unfavourable to their collecting, the bees did not go far from the hives, the darkness, and the chilliness of the air, soon compelled them to return, and probably calmed their agitation, for we saw them quietly reascend their combs, and order appeared to us to be re-established. This moment was taken to close the hive again.

On the 19th, we saw two royal cells begun on one of the combs of the nursery (*couvain*); the evening of this day, and ^{Other royal cells made,} at the same hour as the day before, we heard a great tumult in the closed hive, it was in a general confusion, and we were again obliged to permit the swarm to come out.

The 20th was the fifth day of their captivity; we thought it had been of sufficient duration, and were also very impatient to examine the nursery, and to see what was the cause of the periodical agitation of these bees: Burnens therefore opened the first and twelfth windows, and drove the bees from the combs, suffering them to take their flight in a room, the windows of which were shut.

He first noticed that the royal cells had not been continued, that they did not contain any worm, and that there was not an atom of the jelly which serves for the nourishment and the cradle of the larvæ of the queens.

He fought in vain for eggs, for worms, and for the liquid in the common cells; all had disappeared. Had these worms ^{—but the pro-} ^{cesses d'id not go} ^{on without pol-} ^{died len.}

died of hunger? Had we, by withdrawing the fecundating powder, deprived the bees of every means of nourishing them?

To ascertain this it would be sufficient to restore them their pollen, and observe the issue. The bees were therefore again returned to their prison, after having substituted young worms for those which had been suffered to die.

On the 22d, we found that the bees had fastened these combs, and that they were again in a state of incubation; we then gave them some pieces of combs in which other bees had stored up the fecundating powder, and, the better to observe what they did with it, we took some of the pollen out of the cells, and laid it exposed on the stand of the hive. In a few minutes the bees discovered the pollen in the combs, and that which we had taken out; they took it grain by grain in their jaws, and conveyed it into their mouths; those which had eaten most voraciously re-ascended the combs, and placed themselves, at first, upon the cells of the young worms, which they entered with their heads foremost, and remained there a greater or less length of time. One of the windows of the hive was now opened cautiously, Burnens powdered the bees which eat the pollen, and watched them for some hours; he observed that the marked bees always re-ascended to the nursery, and immediately entered the cells of the young bees.

The 23d, we found the royal cells begun.

The 24th, we drove the bees from off the young worms, and we remarked,

1st, That all of them had the jelly, as in the common hives:

2d, That the worms had grown larger, and were forwarder in their cells:

3d, That others had been shut up again: And,

4th, That the royal cells had been lengthened.

The 25th, we withdrew the pieces of comb which we had placed on the stand, and found that the quantity of pollen was sensibly diminished; we afterwards replaced them in the hive with other cells filled with the fecundating powder.

The 26th, the royal cells had been closed during the night, as well as several of the common ones.

The 27th, I restored these bees to liberty; Burnens examined the cells with the greatest attention, and found jelly in all those which

which still contained worms, but most of them were shut with a lid of wax: he examined some of the latter, and found the worms employed in spinning cocoons of silk.

All the worms had therefore been tended as in the natural hives. In this second trial we did not perceive any disorder in this hive; there had not been the least agitation: it is true some of the working bees attempted to go out in the course of the day, but finding it impossible, they re-ascended the combs quietly, which were never left for an instant. The hive being abundantly supplied with honey, and with the pollen necessary for their young, left them nothing to wish for; and they were still more happy when a queen was born, who afterwards became pregnant, and laid a great number of eggs.

After these two experiments there could be no more doubt that the fecundating dust was the aliment proper for the young bees, and that the want of this substance was the cause of their death, and of the evident anguish of their nurses during their first captivity.

The pollen is therefore the food of young bees.

FOURTH EXPERIMENT.

On Bees deprived of Honey and Pollen, and which it was attempted to feed with Sugar.

I wished to know if it was the saccharine part of the honey which enabled the bees to produce wax.

Exp. IV.
What part of the honey contains the principles of the wax?

Burnens confined a swarm in a glazed hive: one pound of Canary sugar was their sole aliment.

He put a second swarm into another hive, and endeavoured to feed them with very coarse raw sugar; and to obtain a term of comparison, a third swarm was shut up in the same manner, and fed with honey.

The bees of the three hives produced wax; those fed with the different qualities of sugar produced it sooner than the swarm which had only had honey, and they produced it in greater quantity.

A pound of Canary sugar reduced to syrup, and clarified with white of egg, yielded 10 gros, 52 grains, of a wax not so white as that which the bees extract from honey.

Wax made from true sugars of different qualities.

An equal weight of raw sugar gave 22 gros of very white wax.

Maple

Maple sugar produced the same effect. This experiment having been repeated seven times successively, always employing the same bees, we could not doubt that sugar contains the principles of wax, and we concluded that it was the saccharine part of the honey which had this property.

CONCLUSION.

Conclusions:

These observations shew,

1st, That the wax comes from the honey :

2d, That the honey is also a food of the first necessity to the bees :

3d, That flowers do not always contain honey, as has been imagined ; that this secretion is subject to the variations of the atmosphere ; and, that the days in which it is abundant are very rare in our climate :

4th, That it is the saccharine part of the honey which enables the bees to produce wax :

5th, That raw sugar yields more wax than honey, or refined sugar :

6th, That the dust of the stamina does not contain the principles of wax :

7th, That this dust is not the food of the adult bees, and that they do not collect it for themselves :

8th, That the pollen affords the only aliment which is proper for their young, but that this substance must undergo a peculiar elaboration in the stomachs of the bees to be converted into an aliment, which is always appropriated to their sex, their age, and their wants, since the best microscopes do not shew the particles of pollen, or their coverings in the liquor prepared by the working bees.

I shall speak of the economical consequences of these observations on another occasion. By showing the breeders the real wants of the bees, they will be possessed of the means of assisting them in time, in all their necessities, and of preserving them in climates in which nature has not placed them, and in which they could not prosper without our aid.

XII.

An Enquiry concerning the Nature of Heat, and the Modes of its Communication. By BENJAMIN Count of RUMFORD, V. P. R. S. &c. Abridged from the Philosophical Transactions for 1804.

(Concluded from p. 63.)

Exp. 12. TWO equal cylindrical vessels of sheet brass polished very bright, each three inches in diameter and four inches long, suspended by their oblique necks in a horizontal position, being placed on their wooden stands, were filled with water at the temperature of 180° ; and their circular flat bottoms were presented, in a vertical position, to the two balls of the thermoscope, at the distance of two inches.

Exp. 12. Bodies alike in all respects and heated, do, at equal distances, affect the thermoscope equally.

When the two hot bodies were presented, at the same moment, to the two balls of the instrument, or what was still better, when two screens were placed before the two balls, at the distance of about an inch, and, after the hot bodies were placed, these screens were both removed at the same instant, the bubble remained without motion in the middle of the horizontal part of the tube of the instrument.

If the distances of the equally hot bodies were rendered unequal, the bubble always moved towards the most remote of the two; and if a single hot body was presented to one of the balls, the bubble was driven from it, and might have been carried quite out of the tube; which, however, was always avoided, as the instrument would have been by that means quite deranged.

Exp. 13. The flat circular bottom of one of the cylindrical vessels was blackened by holding it over the flame of a wax candle, the other vessel remaining bright as before. Both were then filled with water at 180° , and presented at equal distances to the two opposite balls of the instrument, as described in the last experiment.

Exp. 13. A cylinder blackened with smoke, gives out more radiant heat than a clear metallic surface.

The bubble was instantly driven out of its place by the superior action of the blackened surface; and it did not return to its former station until the blackened surface had been removed to more than eight inches from the ball to which it was

presented; the other, which had not been blackened, remaining in its first position, at the distance of two inches.

The other coatings which had accelerated the cooling, were also found to increase the radiation.

Other similar experiments were made to shew whether those coatings or clothings which, in the former experiments with the large vessels, had accelerated their cooling, did also increase their power of radiation, as shewn by the process and instruments last described. And the results invariably proved that this is the case.

It was also a question deserving to be investigated, whether any peculiarity belonging to the metallic surface hitherto used, namely of brass, might have influenced the results.

Exp. 14, 15.
All metallic surfaces radiate equally.

Exp. 14, 15. The two large cylindrical vessels No. 1 and 2, were covered with a single coating of oil varnish, and on this, when sufficiently dry, was laid a covering of thick gold leaf upon No. 1, and thick silver leaf on No. 2. These vessels were cooled through the interval of 10 degrees, in the same time as the naked vessel used as the standard. Similar experiments with vessels of tinned iron and of lead, shewed that the radiation from all these metals, though so different in their conducting power, is the same.

"Is not this," says the Count, "owing to their being all equally wanting in transparency? And does not this afford us a strong presumption that heat is, in all cases, excited and communicated by means of radiations or undulations, as I should rather choose to call them?"

And he proceeds to observe, that another very important question also must be decided before these points can be determined, and that is, Whether bodies are cooled in consequence of the rays they emit, or by those they receive? The celebrated experiment of Pictet has shewn, in our author's opinion, that rays or emanations proceed from cold bodies, which may be concentrated by concave mirrors, and will affect a delicate air thermometer.

Exp. 16, 17.
A cold body depresses the temperature of other bodies by radiation.
Metal horizontally placed.

Exp. 16, 17. The horizontal cylindrical vessels (Fig. 3, Plate I. of the present volume) were made very clean and bright, were duly fixed, and left for several hours in a room near the thermoscope; and when each vessel was in succession presented to that instrument, with every precaution to prevent irregularity from external circumstances, it was not found to be affected by them. One of the vessels was now taken away and filled with ice and water, and then presented, at the distance

distance of four inches, to one of the balls of the thermoscope. The bubble immediately moved towards the cold body, and passed through the space of one inch. A nearer approach of the cold body produced a still farther motion in the same direction.

Exp. 18, 19. Though this result appeared to the Count to *Exp. 18, 19.* prove indisputably that cold bodies emit rays capable of generating cold in warmer bodies, yet, from the importance of the fact, he chose to vary the substance presented to the instrument, as well as to remove all suspicion of the action of cold currents of air. He therefore laid the thermoscope on one side, and placed underneath one of its balls a solid cake of ice, at the distance of six inches. The result of this experiment was the same as of the other, and the bubble was moved one inch in the tube. Ice-cold water produced the same effect as ice itself.

Ice and ice-cold water underneath the thermoscope, cooled it by radiation. No current of air could here ascend.

Exp. 20. Whether this effect of cold bodies be governed by the same laws as those observed by varying the nature of the surface in heated bodies, or by any other, now remained to be ascertained. It was before found that metal blackened over a candle, did emit much more of calorific rays than the same metal when naked. The same experiment was now made with cold bodies. One of the cylinders had its end blackened, and the other cylinder was left bright and naked. Both were filled with ice and salt, and at the same instant they were suffered to act from equal distances on the thermoscope. The bubble moved towards the blackened body; not indeed so much as when the bodies were heated in the former experiments, because the temperature was not here so far distant from the common temperature of surrounding bodies as in that experiment; but on several repetitions of the experiment with these cold bodies, the effect was constantly the same. It was found that the precipitation of ice out of the surrounding air tended speedily to raise the temperature, and also that the clean surface, when covered with ice, had a greater frigorific effect than when the metal was naked.

Exp. 20. Metal blackened with smoke cooled the instrument more, by radiation, than a clear surface at same temperature.

Ice radiates more than metal.

Exp. 21. The radiation of heat from animal substances appearing, from some facts, to be considerable, a piece of gold-beater's skin was applied wet to one of the vessels, and remained firmly adherent when dry. This, when filled with hot water at 180°, and presented to the thermoscope in opposition

Exp. 21. Gold-beater's skin radiates 25 times more than metal. Hot water in the vessels.

to the other vessel clean and bright, and also filled with the same fluid at the same heat, was found to be much more calorific. The bubble which moved from the coated body, did not return to its station until this body was removed to a distance five times as great as that of the other. Whence our author concludes, that it emitted twenty-five times the quantity of calorific rays.

Exp. 22. Similar effect with ice-cold water. The gold-beater's skin produced more cold by radiation.

Exp. 22. The vessels used in the last experiment were emptied and refilled with ice and water. They were then presented at equal distances from the respective balls; and the effect of the body which was covered with gold-beater's skin, was much more considerable in producing cold.

The radiation of cold bodies appearing to the Count thus to be proved beyond all doubt, he was desirous of ascertaining whether the frigorific rays possess an equal power of affecting the temperature of bodies as the calorific rays do; the temperatures of the radiant bodies being at the same distance each way from the body to be acted upon.

Exp. 23. A hot and a cold vessel equally distant from the common temperature, affect the thermometer equally.

Exp. 23. With this intention, one of the vessels filled with pounded ice and water, was presented to a ball of the instrument, and the other vessel filled with water at 112° , was presented at an equal distance on the opposite side of the same ball; the temperature of the room and instrument being 72° , or 40° distant from each of the temperatures of the vessels, and the other ball of the instrument being defended from all radiation by screens. The bubble remained motionless; so that the opposite actions were in fact equal. And when either vessel was drawn farther off, the effect of that vessel became less, and the bubble moved; that is to say, towards the ball if the cold vessel were nearest, or from the ball if the hotter; and these effects were equal in quantity as well as in celerity of motion.

Why the cooling by radiation has been less noticed.

The Count again repeats his conclusion from these experiments lately exhibited to Professor Pictet, M. de Saussure, and M. Senebier at Geneva, that the rays which generate cold are just as real and just as intense as those which generate heat; and he proceeds to account for this result having been overlooked, by observing, that the degrees of cold we are able to produce, are much less distant from the usual temperature than those of heat, which are within our power. Thus a cannon ball, heated to 160 degrees, or 70 degrees above blood-heat,

heat, would radiate quite as much as ice; and a bullet of freezing mercury would radiate scarcely more than another of boiling water: both which hot temperatures are very trifling in comparison to the heat in which we are habituated to notice and observe this phenomenon.

Exp. 24. After the proof that cold bodies of the same kind affect the thermoscope equally, when equally distant from the common temperature, it remained to be determined whether the different modifications of surface have the same effects in the ready propagation of lower temperatures, as they had shewn before in higher. To shew this, it was only required to oppose them to each other, as to their action upon the same ball, as in the last experiment. With this view both discs were blackened, and, the temperature of the room being 72° as before, one of them was charged with ice and water, and the other with water at 112° . These, at equal distances from the ball as before, did not affect the bubble, and therefore their actions were precisely equal.

Exp. 24. The same surfaces which produce most heat by radiation, do also produce most cold.

In the consideration of two kinds of rays, calorific and frigorific, it did not escape the attention of our author, that hot and cold are terms denoting mere relations; so that the same body will be either hot or cold accordingly as the common temperature, or temperature of the bodies of comparison, is lower or higher. Questioning, therefore, as to the difference between calorific and frigorific rays, he demands whether the same rays may not be either calorific or frigorific, accordingly as the body at whose surface they arrive, is hotter or colder than that from which they proceed?

Do not the same rays produce heat or cold, as the temperature of the emanating bodies is higher or lower than the receiving body,

Exp. 25. The whole of the external surface of one of the large cylindrical passage thermometers, *Fig. 1, Plate I.* was covered with gold-beater's skin, and, along with the standard instrument of the same kind, was filled with hot water. The covered instrument cooled through the standard interval of ten degrees, namely, from $101\frac{1}{2}$ to $91\frac{1}{2}$, in twenty-seven and three quarter minutes; but the naked instrument employed 45 minutes in passing through the same interval.

Exp. 25. Metal covered with gold-beater's skin cools faster.

Exp. 26. Both instruments were suffered to remain in the cold all night, when the temperature in the naked instrument was $50\frac{1}{2}^{\circ}$, and that of the covered instrument $49\frac{1}{2}^{\circ}$; the air of the room being 48° . Both instruments were then removed into a warm room, of which the temperature continued between

Exp. 26. And also acquires heat more quickly.

tween 64° and 65° . The covered instrument acquired heat the most rapidly; for in a quarter of an hour both stood at $51\frac{1}{2}^{\circ}$, and at the end of four hours the naked instrument shewed the temperature 61° , and the covered $63\frac{1}{2}^{\circ}$: whence the Count observes, that those substances which part with heat the most readily, have also the greatest facility to acquire it.

Rays received as well as those emitted, alter the heats of bodies.

Radiation does not heat the air.

Steady temperature of animals,

may be affected by radiation.

Instance in negroes.

Exp. 27. Black paint upon animal membrane increases radiation. Emission,

and also reception.

He then proceeds to reason on the probability that the temperatures of bodies may be changed, not only by the rays they emit, but by those they receive from other bodies; and as the cooling of hot bodies is so much accelerated by covering their surfaces with substances which affect the radiation or absorption, he thinks it highly probable that the air is but little affected as to its temperature, by these rays which pass through it; and he contemplates the establishment of this supposition, as promising to explain various interesting phenomena; particularly that of the steady temperature of living animals, notwithstanding the great quantity of heat generated in their lungs, and the different temperatures of the fluid which surrounds them.

For it is evident, the greater power an animal may possess of throwing off heat by radiation, independently of the effect of the contact of the surrounding air, the less will his temperature be affected by the changes in that fluid, or the oppressive heat of the climate. The probability that negroes and people of colour, who support the heats of tropical climates much better than white people, offers itself in this place as the consequence of their colour; a quality which not only enables them to throw off heat, but even, as the Count much suspects, to absorb frigorific rays from such bodies as may emit them.

Exp. 27. When the flat ends of both the horizontal cylindrical vessels were covered with gold-beater's skin, and one of them painted black upon the covering with Indian ink, this last, as indicated by the thermoscope, emitted more calorific rays from the included hot water than the other vessel did.

Exp. 28. When the same two vessels filled with boiling-hot water, were set to cool, the blackened vessel cooled through the standard interval of ten degrees in $23\frac{1}{2}$ minutes, while the other, which was not blackened, employed 28 minutes.

The author again reverts to the application of these facts to animal bodies. Whether the oily coating which savages apply to their skins in cold climates, may not add to their comfort by reflecting frigorific rays; whether the Hottentots, still more disgustingly besmeared, may not derive advantages similar to those derived by negroes from their black colour? are questions that promise to lead to results of practical value. He then proceeds to explain more fully the manner in which negroes may be supposed to resist the action of a burning sun. An oil exudes from the skin of these people when exposed naked to the sun; which oil reflects the sun's calorific rays. Heat more intense produces sweat, which not only aids the former process, but generates cold by evaporation. But when the sun is set, the oil retires from the surface, and the skin becomes well adapted to admit frigorific rays from the neighbouring bodies.

Exp. 29. The thermoscope was perceptibly affected by the radiation of cold bodies: It was desirable to know whether this effect would be shewn in a grosser way, by accelerating the cooling of a hot body. For this purpose, two conical vessels of thin sheet brass, each four inches diameter at the base, and four inches high, ending above in a cylindrical neck, were separately enclosed in a cylinder of thin paste-board covered with gilt paper, and then the vessels were covered up with rabbit-skins having the hair on them, in such a manner that no part of these vessels, except their flat bottoms, was exposed naked to the air. The bottoms were covered with gold-beater's skin painted black with Indian ink, in order to render them as sensible as possible to calorific and frigorific rays.

Effects of the oil on the skins of Hottentots, &c. and of the natural changes in the skins of negroes.

Exp. 29. Vessel of water more rapidly cooled by radiation from ice.

The two vessels thus prepared were suspended, with their bottoms downwards from the arms of a stand, and under each was placed a pewter platter blackened on the inside by smoke from a candle. The platters themselves were supported on shallow earthen dishes which rested upon wooden stands; each pewter platter having a perforated cover of thick paper, in the center of which was a hole six inches in diameter. The distance from the floor of the room to the smoked surface of each platter was 40 inches, and the interval between the surface of each conical vessel and its correspondent platter immediately beneath, was four inches. One of the platters remained at the tempera-

ture

ture of the room, but the other was kept constantly ice-cold by means of pounded ice and water contained in the dish beneath it.

The two conical vessels were now filled with boiling hot water, and every precaution was taken to prevent agitation in the air; the only means by which an ascent of cold fluid could be suspected. The result of the experiment was, that the vessel suspended over the ice-cold platter cooled from 50° to that of 40° above the temperature of the room, in 33 minutes and 42 seconds; whereas the other vessel required 39 minutes and 15 seconds to cool through the same interval.

**Exp. 30. Repe-
tition.**

Exp. 30. Upon repeating this experiment the next day, the times were 33 minutes 15 seconds and 39 minutes 30 seconds.

**Velocity of cool-
ing accelerated
by the radiation
from ice, in the
ratio of 5 to 4.**

As the cooling of these vessels is a complicated process which includes the consideration of the heat that passed through the covered sides, in these different times; the Count enters into a process of computation, founded on the principles made use of at page 61, by which he determines that the velocity with which the heat passed through the bottom of the vessel exposed to the ice, was to that with which it passed through the bottom of the other vessel as five to four nearly.

**Exp. 31. Pro-
cess to shew the
heat absorbed by
the air when a
current was al-
lowed to take
place. It was
then one twenty-
seventh of the
whole loss.**

Exp. 31. By the manner of defending the two conical vessels each had a circular band or hoop of the fine post paper which projected half an inch below its base. It is evident that the air in this space could not pass upwards, as it became heated by contact, and consequently, that very little of the cooling effect could have been produced by the contact of an ascending current. To ascertain what this effect might have been, the two vessels were suspended as before; but one of them had its base inclined in an angle of 45° , while the other base continued horizontal. In this situation they were filled with boiling water, and suffered to cool without the platter and stands beneath them. Two effects would follow from these arrangements; the vessel, which in the other experiment had been placed over a platter at the temperature of the room, would now cool a very little faster by the absence of that platter, which no doubt must have had its temperature a little raised; and the inclined vessel in the present experiment would be cooled somewhat more speedily, by the successive contact of the ascending current of air which was at liberty to rise. This vessel was found to cool through the standard interval of 10° in $37\frac{1}{4}$ minutes, and the
horizontal

horizontal vessel employed 38½ minutes. By referring these facts to computation, the Count finds that the heat lost by actual communication to the air, is nearly $\frac{1}{27}$ part of the whole loss.

Exp. 32. The counter radiation by the platter, which was stated under the last experiment as impeding the cooling process, affords the important prospect of explaining the effect of clothing, and therefore deserved to be more fully examined. The experiments 29 and 30 were therefore repeated, with the distance of three inches only between the bottom of each vessel and its corresponding platter. The times of cooling through 10° , were now $33\frac{1}{2}$ minutes and $40\frac{1}{2}$ minutes.

Exp. 32. Velocity of cooling retarded by the vicinity of other bodies. Hence clothing.

Exp. 33. And when the distance was diminished to two inches, the times proved $32\frac{1}{2}$ minutes and $42\frac{1}{2}$ minutes.

Exp. 33. Repetition.

These experiments shew that the vicinity of a cold body, of which the low temperature is not kept up by artificial means, retards the cooling of a hot body. And from this fact the Count concludes, that if the hot body had been a globe suspended in the center of another larger thin hollow sphere, of the same temperature, at the commencement, as the air and walls of the room, the cooling would have been more slow than if the external globe had not been present; and also, if the external globe itself were included in another globe of the same description, the retardation would have been still more considerable. And by extending this supposed experiment to a number of thin concentric hollow spheres, we may conceive a great retardation to follow, and shall become acquainted with the nature of the effects which take place when a hot body is surrounded by proper clothing.

The vicinity of many thin bodies in succession will constitute a clothing:

If the spheres were metallic, the cooling would be slower more effectual if the surfaces were polished than if unpolished or blackened; the surfaces be polished. whence it is highly probable, that the warmth of any clothing depends very much upon the polish of its surface.

*The microscope shews that those substances which supply Furs, feathers, us with the warmest coverings, such as furs, feathers, silks, ^{silks, &c. are} highly polished, and the like, have their surfaces highly polished; and the finer the fibres, or the greater number of interposed polished surfaces, the warmer is the clothing.

In the former experiments of Count Rumford, he considered the warmth of clothing as principally depending on the obstacles it opposes to the motion of the surrounding cold air; but

but, by a patient and careful examination of the subject, he is convinced that the efficacy of radiation is much greater than he had supposed; from the result of the experiment No. 31, it appears that a very small part of the heat of a body cooled in the air, is in fact communicated to that fluid; much the greatest portion being communicated to surrounding bodies at a distance; and, in one of his former experiments, a hot body was cooled, though it was placed in a torricellian vacuum.

Air heated only at its surface and by radiation.

He considers the heat which air receives by coming in contact with a hot body, to be communicated by radiation, in the same manner as it is received by other bodies at a greater distance; and he apprehends that the contiguous particle receives the heat in preference for no other reason than because it is at the surface of the fluid, this being the place where reflection, refraction, and increase of temperature, take place; and, from these considerations, he explains what has been called the non-conducting power of transparent fluids.

Analogy between light and heat.

By extending the analogy of those facts which we know concerning the effect of polished surfaces on light, to the radiations of heat also, the preceding facts are easily explained. The frigorific rays are reflected externally, and a large portion of the calorific rays, which would have issued forth through a rough surface, are, in the other case, turned inwards by reflection.

A drop of water resists the heat of ignited iron, by its polish.

The polished surface of a drop of water, which rolls about at a distance from the face of a piece of red-hot iron, enables it to reflect the calorific rays; the water acquires little heat, and is evaporated slowly.

With a less heat the water enters the pores of the oxide upon the metal, loses its polish, acquires heat very rapidly, and is soon evaporated.

It retains its polish longer in silver, and resists more.

If the metal be less oxidable, as, for example, a silver-
spoon, the drop of water will support or resist a lower heat. In fact it does not so soon lose its polish; but at a still lower heat, that is to say, a little above boiling-water, a drop of water is instantaneously evaporated.

Exp. 34. Water rolling in a spoon blackened with smoke, cannot be made to boil.

Exp. 34. A clean polished spoon, rendered black by holding it over the flame of a wax candle, will receive a large drop of water, which will roll about without wetting the blackened surface. This drop cannot be made to boil by holding the spoon over the flame of a candle. When the
spoon

spoon is too hot to be held by its handle, the drop of water poured into the palm of the hand is warm, but by no means scalding hot.

Exp. 35. If a large drop of water be formed at the end of a small splinter of light wood, and the drop be thrust quickly into the center of the flame of a newly snuffed candle, it will remain for a considerable time in the center of the flame, without being apparently affected by the heat; and if it be taken out of the flame and put upon the hand, it will not be found to be scalding hot. If it be held for some time in the flame, it will be gradually diminished by evaporation; but it does not appear that the heat is communicated by the flame, but by the wood to which it adheres, which is soon heated, and at last set on fire.

Exp. 35. Drop of water put into the flame of a candle, is not heated.

The remainder of the Count's memoir consists of theoretical remarks and inferences, occupying 29 pages of the Transactions. I have endeavoured faithfully to describe the facts in the way of abridgement, but cannot with the same facility do justice to these argumentative results. I shall therefore, for the present, conclude my account of his paper; but may not perhaps wholly overlook his theories upon some future occasion.

Why the theoretical remarks are not here abridged.

XIII.

Letter from Professor VEAU-DE-LAUNAY to J. C. DELAMETHERIE, on fulminating Silver.*

AS it is at all times useful to state facts, whatever may be the results, I think it right to inform you of an accident which occurred in my laboratory.

Accident with fulminating silver.

I had employed one of my pupils, a very good operator, to prepare a small quantity of fulminating silver, which he executed with skill.

The quantity obtained was about five grains, or a quarter of a gramme: it was deposited in a crystal capsule about two lines in thickness. He had taken a small quantity, about half a grain, which was separated with a card, and then dried, and

* From Journal de Physique, &c. Floreal, An. XII.

afterwards

afterwards detonated by slight friction. Next day, that is to say twenty-four hours after the preparation, this young person was desirous of taking an equal quantity from the capsule to repeat the experiment, but he had scarcely touched the preparation with the corner of a card when a violent detonation and explosion took place in the capsule which was shattered into a thousand pieces. His face was covered with the vaporised preparation, which was almost black, and adhered strongly to the skin: his eyes experienced a strong shock, which produced extreme pain; the opaque cornea became red and inflamed. Happily his fear was the greatest evil: by washing and bathing his eyes and face frequently with cold water, the effects of the detonation were soon dissipated.

Fortunately, none of the fragments of the glass had touched his eyes or his face; they were thrown nearly in a horizontal direction, to a considerable distance: Some were thrown upwards of twelve feet.

As the effects of this preparation may have more calamitous consequences. I think it useful to be guarded against the dangers which it may occasion.

XIV.

Pyrotechnic Observations, with their Application to evaporating Furnaces. By Cit. CURAUDAU, Corresponding Member of the Apothecaries Society of Paris, and Resident Associate at the Athenæum of Arts.*

SINCE I published my observations on the causes of the imperfections of evaporating furnaces, I have had occasion to make others on the same subject, the application of which may add to the advantages in their construction which I have made known.

Disadvantages in the construction of evaporating furnaces, &c. In my first memoir, I proved that the bottom of the copper in the evaporating furnaces, not only obstructs the elevation of the temperature, but also diminishes the activity of the fire, and is rather favourable to the gazification of the combustible body, than to its oxigenation: I cited the lamp of Argand with its

* From *Annales de Chimie*, No. 149, Floreal, An. XII.

glass chimney, as an example of the necessity of raising the temperature round the combustible, whenever a complete and powerful combustion is required. At present, I shall take the enameller's lamp as an example in support of my new observations: I may say that I am indebted to the examination of its effects, for those which it has led me to make. In fact, if the jet of the flame of an enameller's lamp be examined, it will be found that the intensity of its action depends on the current of air which is directed on the flame of the wick; it will also be seen, that it is only at the extremity of its jet that the greatest energy of the calorific rays exists, and that its intensity is such that, by means of this lamp, effects may be produced, which are, comparatively, as powerful as those obtained in our best furnaces.

Effects of the current of air on the flame of Argand's lamp.

This mode of action of the caloric proves, therefore, that its effects may be augmented, by augmenting the rapidity of its current, and by directing it skilfully upon the body to be heated. These are the conditions which I have endeavoured to unite in my new construction, and which, agreeably to the application I have made of them, are employed to support an opinion which required an experiment on a large scale to escape being placed in the class of hypotheses.

Having been lately consulted upon the construction of a brewer's furnace, I took the opportunity to show the advantages of the alterations which my observations appeared to me to render necessary, and to induce the proprietor to construct his furnace according to the plan I sent him.

applied to works on a large scale.

The following is the result of the experiments which were made to ascertain the advantages possessed by the new furnace over that which it replaced.

Comparison of two furnaces.

The old furnace required $2\frac{1}{2}$ hours to raise the temperature of 2500 litres of well-water to 50° of Reaumur, and consumed in the operation, which was repeated daily, 625 kilogrammes of new dry wood.

The present furnace, on the contrary, consumes only 450 kilogrammes of wood in the same operation, and is only one hour in raising the temperature of 2500 litres of well-water to 50° : Whence it results, that this new construction evidently makes a saving of $\frac{1}{4}$ in time and nearly one third in the combustible.

Such

Such advantages appeared to me to be of sufficient importance to deserve to be known, and to make it desirable that advantage may be taken of a new process which will have great influence on the economy of the fuel necessary in manufactures.

Description of the Furnace. Plate

*Description of
the new furnace.*

A. Opening of the fire-place: it is 16 inches wide and 13 high.

B. This part of the furnace resembles a vault: It is $21\frac{1}{2}$ inches in height, $31\frac{1}{2}$ in width, and 5 feet in depth. To add to the effect produced by the heat, I give the middle of the fire place a depth of 2 inches more than at its sides, which makes it preceptibly concave.

C. An opening made in the middle of the vault, and which is intended to increase the rapidity and the action of the calorific rays; it is 5 inches high, and is equal in thickness to the vault. In its lower part, this opening is $23\frac{1}{2}$ inches long, and $17\frac{1}{2}$ wide; and, in its upper part, is $19\frac{1}{2}$ long, and $13\frac{1}{2}$ wide, which gives each extremity of the opening the form of a spheroid whose longest axis is in the direction of the length of the vault.

D. The distance of the copper from the orifice for the heat is 5 inches in the middle, and is reduced to 4, at the angle E, which gives the advantage of concentrating the calorific rays, in proportion as they lose their intensity by their expansion.

From the angle E to that at F, there is a distance of 15 inches: the re-entering angle of F is $2\frac{1}{4}$ inches from the copper, and its salient angle only one inch.

G and H are angles similar to that at F, but which are 11 inches distant from each other: these angles may be multiplied according to the height of the copper. The advantages which they give consist in making the calorific stream undergo several breaks, which increases their power at the place of their deviation.

I is an opening communicating with the chimney: it is 27 inches wide and 5 inches high. At the angle H, half the circumference of the furnace should be closed by a row of bricks, for the purpose of forcing the heat to direct itself from
the

the side opposite to the chimney; immediately above this course of bricks, those which succeed must be removed four inches from the copper, and continue so to the height of four inches; afterwards each course of bricks must be brought nearer, so that this reservoir of heat may be closed at the height of five inches, which must be done round all the circumference of the copper.

K is the opening of the chimney: it must be three decimetres square through all its height, and at least four metres high.

L is a blower of hammered iron; it is one metre above the copper, and serves to open or close the chimney at pleasure.

The proportions laid down in this plan are intended for a copper four feet three inches wide in its lower part, and forty inches deep.

XV.

*An Account of a curious Phenomenon observed on the Glaciers of Chamouny; together with some occasional Observations concerning the Propagation of Heat in Fluids. By BENJAMIN Count of RUMFORD, V. P. R. S. Foreign Associate of the National Institute of France, &c. &c.**

IN an excursion which I made the last summer, in the month of August, to the Glaciers of Chamouny, in company with Professor Picquet of Geneva, I had an opportunity of observing, on what is called the Sea of Ice, (*Mer de Glace*), a phenomenon very common, as I was told, in those high and cold regions, but which was perfectly new to me, and engaged all my attention. At the surface of a solid mass of ice, of vast thickness and extent, we discovered a pit, perfectly cylindrical, about seven inches in diameter, and more than four feet deep; quite full of water. On examining it on the inside, with a pole, I found that its sides were polished; and that its bottom was hemispherical, and well defined.

This pit was not quite perpendicular to the plane of the horizon, but inclined a little towards the south, as it de-

Cylindrical pit
(in a mass or sea
of ice) contain-
ing water.

* Phil. Transf. 1804, p. 23.

scended;

scended; and, in consequence of this inclination, its mouth or opening, at the surface of the ice, was not circular, but elliptical.

They are frequently found.

From our guides I learnt, that these cylindrical holes are frequently found on the level parts of the ice; that they are formed during the summer, increasing gradually in depth, as long as the hot weather continues; but that they are frozen up, and disappear, on the return of winter.

Inference against the conducting power of water.

I would ask those who maintain that water is a conductor of heat, how these pits are formed? On a supposition that there is no direct communication of heat between neighbouring particles of that fluid, which happen to be at different degrees of temperature, the phenomenon may easily be explained; but it appears to me to be inexplicable on any other supposition.

The quiescent mass of water, by which the pit remains constantly filled, must necessarily be at the temperature of freezing; for it is surrounded on every side by ice: but the pit goes on to increase in depth, during the whole summer. From whence comes the heat that melts the ice continually at the bottom of the pit? and how does it happen, that this heat acts on the bottom of the pit only, and not on its sides?

Solution of the effect by the greater density of water above snow.

These curious phenomena may, I think, be explained in the following manner: The warm winds which, in summer, blow over the surface of this column of ice-cold water, must undoubtedly communicate some small degree of heat to those particles of the fluid with which this warm air comes into immediate contact; and the particles of the water at the surface so heated, being rendered specifically heavier than they were before, by this small increase of temperature, sink slowly to the bottom of the pit; where they come into contact with the ice, and communicate to it the heat by which the depth of the pit is continually increased.

This operation is exactly similar to that which took place in one of my experiments, (See my Essay on the Propagation of heat in Fluids, *Experiment 17*.) the results of which, no person, to my knowledge, has yet explained.

Constant temperature of water at the bottom of deep lakes, ascertained in support of the non-

There is another very curious natural phenomenon, which I could wish to see explained in a satisfactory manner, by those who still refuse their assent to the opinions I have been led to adopt, respecting the manner in which heat is propagated in

in fluids. The water at the bottoms of all deep lakes is constantly at the same temperature, (that of 41° Fahrenheit,) summer and winter, without any sensible variation. This fact alone appears to me to be quite sufficient to prove, that if there be any immediate communication of heat between neighbouring particles or molecules of water, *de proche en proche*, or from one of them to the other, that communication must be so extremely slow, that we may with safety consider it as having no existence; and it is with this limitation that I beg to be understood, when I speak of fluids as being non-conductors of heat.

In treating of the propagation of heat in fluids, I have hitherto confined myself to the investigation of the simple matter of fact, without venturing to offer any conjectures relative to the causes of the phenomena observed. But the results of recent experiments on the calorific and frigorific radiations of hot and of cold bodies, (an account of which I shall have the honour of laying before the Royal Society in a short time,) have given me some new light respecting the nature of heat, and the mode of its communication; and I have hopes of being able to show *why* all changes of temperature, in *transparent* liquids, must necessarily take place at their surfaces.

I have seen with real pleasure, that several ingenious gentlemen, in London, and in Edinburgh, have undertaken the investigation of the phenomena of the propagation of heat in fluids; and that they have made a number of new and ingenious experiments, with a view to the farther elucidation of that most interesting subject. If I have hitherto abstained from taking public notice of their observations on the opinion I have advanced on that subject, in my different publications, it was not from any want of respect for those gentlemen, that I remained silent, but because I still found it to be quite impossible to explain the results of my own experiments, on any other principles than those which, on the most mature and dispassionate deliberation, I had been induced to adopt; and because my own experiments appeared to me to be quite as conclusive (to say no more of them) as those which were opposed to them; and, lastly, because I considered the principal point in dispute, relative to the passage of heat in fluids, as being so clearly established by the circumstances attending several great operations of nature, that this evidence did not appear to me to be in

Notice of experiments against this doctrine.

danger of being invalidated by conclusions drawn from partial and imperfect experiments, and particularly from such as are allowed on all hands to be extremely delicate.

Heat of the sides
of the vessel,
and descending
current in a ves-
sel of ice.

In all our attempts to cause heat to descend in liquids, the heat unavoidably communicated to the sides of the containing vessel, must occasion great uncertainty with respect to the results of the experiment; and, when that vessel is constructed of ice, the flowing down of the water resulting from the thawing of that ice, will cause motions in the liquid, and consequently inaccuracies of still greater moment, as I have found from my own experience; and, when thermometers immersed in a liquid, at a small distance below its surface, acquire heat, in consequence of a hot body being applied to the surface of the liquid, that event is no decisive proof that the heat acquired by the thermometer is communicated by the fluid, from above, downwards, from molecule to molecule, *de proche en proche*; so far from being so, it is not even a proof that it is from the fluid that the thermometer receives the heat which it acquires; for it is possible, for aught we know to the contrary, that it may be occasioned by the radiation of the hot body placed at the surface of the fluid.

Reference to the
experiments of
boiling water
standing over
ice.

In the experiments of which I have given an account, in my Essay on the Propagation of Heat in Fluids, great masses, many pounds in weight, of boiling hot water, were made to repose for a long time (three hours) on a cake of ice, without melting but a very small portion of it; and, on repeating the experiment with an equal quantity of very cold water, (namely, at the temperature of 41° Fahrenheit,) nearly twice as much ice was melted in the same time. In these experiments, the causes of uncertainty above mentioned did not exist: and the results of them were certainly most striking.

The conclusions which naturally flow from those results, have always appeared to me to be so perfectly evident and indisputable, as to stand in no need, either of elucidation, or of farther proof.

If water be a conductor of heat, how did it happen that the heat in the boiling water did not, in three hours, find its way downwards, to the cake of ice, on which it reposed, and from which it was separated only by a stratum of cold water, half an inch in thickness?

I wish

I wish that gentlemen who refuse their assent to the opinions I have advanced respecting the causes of this curious phenomenon, would give a better explanation of it than that which I have ventured to offer. I could likewise wish that they would inform us how it happens, that the water at the bottoms of all deep lakes remains constantly at the same temperature: and, above all, how the cylindrical pits, above described, are formed in the immense masses of solid and compact ice which compose the Glaciers of Chamouny!

A remark, which surprised me not a little, has been made by a gentleman of Edinburgh, (Dr. Thompson,) on the experiments I contrived, to render visible the currents into which liquids are thrown on a sudden application of heat, or of cold. He conceives, that the motions observed in my experiments, among the small pieces of amber which were suspended in a weak solution of potash in water, were no proof of currents existing in that liquid; as they might, in his opinion, have been occasioned by a change of specific gravity in the amber, or by air attached to it. I am sorry that so mean an opinion of my accuracy as an observer should have been entertained, as to imagine that I could have been so easily deceived. For nothing surely is easier, than to distinguish the motion of a solid suspended in a liquid of the same specific gravity, which is carried along by a current in the liquid, from that of a body which descends, or ascends, in the liquid, in consequence of its relative weight, or levity. In the one case, the motion is uniform; in the other, it is accelerated. In a current, the body may be carried forward in all directions, and even in curved lines; but, when it falls in a quiescent fluid, by the action of gravity, or rises, in consequence of its being specifically lighter than the fluid, it must necessarily move in a vertical direction.

The fact is, that I very often observed, in the course of my numerous experiments, the motions of small particles of matter, of different kinds, in water, which Dr. Thompson describes; but, so far from inferring *from them* the existence of currents in that fluid, their cause was so perfectly evident, that I did not even think it necessary to make any mention of them.

I cannot conclude this Paper, without requesting that the Royal Society would excuse the liberty I have taken in troubling

them with these remarks. Very desirous of avoiding every species of altercation, I have hitherto cautiously abstained from engaging in literary disputes; and I shall most certainly endeavour to avoid them in future.

I am responsible to the public for the accuracy of the accounts which I have published of my experiments; but it cannot reasonably be expected, that I should answer all the objections that may be made to the conclusions which I have drawn from them. It will however, at all times, afford me real satisfaction to see my opinions examined, and my mistakes corrected; for my first and most earnest wish is, to contribute to the advancement of useful knowledge.

XVI.

Account of two Sketches; viz. one for a perpetual Motion, and the other of a jointed Parallel Rule, which has no side Deviation. In a Letter from R. B.

To Mr. NICHOLSON.

SIR,

Perpetual motions described in the Philosophical Journal.

I was much gratified several years ago by some essays with which you obliged your readers upon the perpetual motion. In the first volume, p. 375, of your quarto series, I find an account of several schemes (necessarily abortive) for producing perpetual motions by the action of gravity, and in your third volume, I find an account of various methods of keeping up the motion of a machine by means of the changes which take place in the barometer and thermometer. I have ventured to send you the sketch of a project for a perpetual motion of the latter kind, which has long remained among my memorandums. You will see that it is not of the class of perpetual motions properly speaking, but merely the application of some existing intermittent motions in nature to the purpose of maintaining the rotation of machinery. If you should hesitate about inserting this, I think the other sketch which accompanies it of an useful instrument, cannot fail to meet your approbation.

Description of a wheel kept in motion by the marine barometer.

Fig. 1, Plate XII. A represents the marine barometer of Halley, but varied by the addition of a vessel B at the open end, in which the water or other fluid, exposes a surface nearly equal

equal to that in the closed vessel A. These two vessels are connected by a long horizontal tube G. It is evident that any change, either in the pressure of the external air or the elasticity of the internal air, will cause the fluid to run along the tube, and add to the weight of A or of B according to circumstances. The heavier vessel will preponderate; but it will be prevented from descending too far by a stop or bearing to which it will arrive. Any change in the inclination of G will move the attached lever C D; by means of which, one of the two horizontal racks will be made to push round that ratchet wheel into which its teeth fall, at the same time that the other rack will be drawn backwards upon its wheel. The opposite action will drive forward the other wheel; and as both these wheels are fixed on the same axis, the system will be driven the same way by every change of density or weight in the air that takes place.

The other instrument, *Fig. 3*, is a parallel rule which is framed without any sliding work, and opens to a much greater extent than usual, without any side deviation. I do not know the inventor; but it was communicated to me by a private hand. The jointed parts are represented in *Fig. 2*, and the dimensions are expressed by the small numerals. From the joint D to the joint E upon the bar, C E, the distance, is = 1; from the joint E to the joint A, = 2; from the joint A to the joint B, = 3; and from the joint B to the joint D, = 4; which is also equal to the part D C of the Bar E C. Now it is found, and I must leave the mathematical proof to your readers, that the point C moves by the opening from B (upon which it lies when shut) in a strait line, very nearly at right angles with B A. From this property it is easily seen, that two parallel rules, having the system of four bars fixed to each, and connected as in the figure, will be moveable without side deviation, and will preserve their parallelism in all situations.

I am Sir,

Your obliged humble servant,

R. B.

XVII.

Familiar Account of the Method of estimating the Value of a Steam-Engine in Horse-Powers as they are called. By a Correspondent.

To Mr. NICHOLSON.

SIR,

Introductory
letter.

AS your excellent Journal is the repository for useful information, whether scientific or practical, I have thought I should oblige many manufacturers and others of your readers, by sending you a very clear report about horse-powers, which a friend of mine has communicated to me, and was received by him from an eminent character in answer to an enquiry professionally made.

It clearly appears from this paper, that the calculation by horse-powers must be fallacious, unless engineers could agree as to the quantity of work they would arbitrarily, in the first instance, ascribe to one horse; and then the expression would be nugatory. And not only so, but it would not then be true that the value of a steam-engine in work, however clearly expressed in quantity *per* day, would be fairly shewn, unless the wages or food of the working being were taken into the account. Coals may be stiled the food of a steam-engine, and nothing is more evident than that, if two engines raise equal quantities of water *per* hour, but consume different quantities of coal, they will not be equally beneficial to the proprietors. I would therefore propose, that the estimate should be made by attending to these two particulars only, and saying nothing about horses, at least in specific arguments intended to have legal effects: And, as a supplement to the facts and observations contained in the report, I will add, that one of the best engines of Boulton and Watt, has been known to raise between 28 and 30 millions of pounds of water to the height of one foot with one bushel of good coals, which appears to be an outside measure; and that, though there are subsequent improvements both in the construction of furnaces and the working gear, yet there are some among late engines which fall short of 20 millions.

I am, Sir,

Your constant reader,

E. T.

Report

*Report concerning the Power of a Steam-Engine erected by Con-
tract at * * * *.*

IT is required to determine whether the steam-engine erected at * * * * by Mr. —, be equal to the power of sixteen Steam engine at * * * *. horses. The same has a cylinder of $21\frac{1}{8}$ inches diameter, and gives 23 double strokes *per* minute, of four feet each.

In answer to this question it must be previously remarked, that steam-engines having originally been recommended and substituted instead of horses, the method of computing by the number of those animals intended to be supplied by means of this invention, has been generally applied, though it is much less certain and accurate than other methods well known to mechanical men. The uncertainty of calculating by horse-powers arises from various causes; such as the great differences of ability between the strong and heavy horses used in London, and those of not half the strength used in various parts of the country; the greater or less degree of speed during work; the quantity of re-action against which they are urged to pull; the shorter or longer time of work; their food, stabling, &c. &c. And this uncertainty, as may easily be conceived, is so great, that the words *horse-power* cannot practically be applied, otherwise than to denote a certain quantity of mechanic effect agreed upon and understood between engineers, and must not be understood to denote any elementary measure, capable of being worked out or inferred within any reasonable or useful limits, from the real power of the horse himself. Reason why steam engines have been compared with horses. It is an uncertain method.

It therefore follows of necessity, that the engine must be examined by first stating its mechanic effect; that is to say, how many pounds weight it is capable of raising through a given space in a given time, that is to say, through the height of one foot during one minute, and then dividing this sum by the like effect producible by one horse, according to the statements and practice of engineers of the first reputation. I confine myself to Mess. Boulton and Watt, Dr. Defaguliers and Mr. Smeaton. Words horse-power must denote an arbitrary effect.

The practice of Mess. Boulton and Watt is, to consider a horse as capable of raising a certain weight, which is stated to be of 32000 pounds avoirdupois, one foot high in one minute. Quantities of work per horse, by Boulton and Watt, Defaguliers and Smeaton. Defaguliers' results brought to the same form, give 27500 pounds;

pounds; and Smeaton's 22916 pounds, under the same circumstances. The lowest of these performances are more than equal to the average power of a horse employed in husbandry for eight hours *per* day.

A steam engine
computed.

If the diameter of the cylinder be multiplied by itself ($21\frac{1}{8} \times 21\frac{1}{8}$), the product will give $446\frac{1}{4}$ round inches for the whole surface: and Smeaton reckons the effective or working pressure *per* round inch on the atmospheric engine, at seven pounds avoirdupois. It is usual to reckon the working pressure on a close engine (like that in question), at 10 pounds the round inch; but I shall first take the seven pounds as being against the builder: So that, by multiplying the round inches $446\frac{1}{4}$ by 7, we have $3123\frac{1}{4}$ pounds for the weight raised. But the strokes are 23 of four feet double *per* minute, that is to say, 184 feet. Multiply the weight $3123\frac{1}{4}$ by the height 184, and the product 574760 will be the mechanic effect of the engine, or the number of pounds it will raise one foot high in one minute. Lastly, Divide this by Boulton and Watt's horse-power ($32\frac{2}{3}$), and the quotient $17\frac{1}{8}$, or very nearly 18, will express the power of the engine in horses.

If we follow Defaguliers, the engine will prove equal to 21 horses.

And, according to Smeaton, its power will be equal to 25 horses.

If we had taken the pressure at 10 pounds *per* round inch, the powers would have proved much greater, as below.

Steam engine
can work twelve
days.

In the above calculations the horses are supposed to be fairly worked, and the engine is supposed to be stopped as soon as the horses leave off. But an engine can work the whole 24 hours; and Smeaton, considering that three sets of horses must be kept to work constantly for the same time, reckoned a steam engine to be equivalent to three times as many horses as could equal its rate of working. The following table will shew the powers of this engine, according to all these several methods:

	Power in Horses according to Boulton and Watt.	Ditto according to Defaguliers.	Ditto according to Smeaton.	24 hours work according to Smeaton in horses.
7lb. presture per round inch.	17 ⁹⁶ / ₁₀₀ or 18 very nearly	21	25	75
10lb. presture per round inch.	25	30	35 ¹ / ₄	107

XVIII.

Experiments proving the Necessity of atmospherical Oxigen in the Process of Vegetation. In a Letter from Mr. JOHN GOUGH.

To Mr. NICHOLSON.

SIR,

DR. Priestley and M. Ingenhoufs have made many experiments with plants, confined in azote; from which philosophers have inferred, that vegetables differ from animals in not requiring the presence and support of atmospherical oxigen. One circumstance however occurs in all these experiments, if my memory be correct, which renders the conclusion doubtful: the plants, used in them were in contact with the water that confined the azote in the inverted jars, through the medium of which they had evidently the same indirect communication with the atmosphere that fishes are known to require.

Reflections on former experiments of the kind.

With a view to throw some additional light upon this disputable point in the vegetable economy, I determined to surround certain succulent plants entirely with azote; which possess the singular property of vegetating when suspended by the roots in dry air. My method of performing this consists in fixing the subject of the experiment by means of a thread, to a small perforated board; standing on a pillar, which was furnished with a foot of lead. The plant being thus secured, was introduced through water into a glass jar of azote; where it was wholly surrounded by the gas.

Experiments on succulent vegetables.

Exp. 1. The first experiment, I made in this manner, was tried in June 1800. A plant of sedum acre was surrounded by azote when it was ready to expand its flowers, and another was placed beside it, under a similar jar; which contained

Experiment with sedum acre going to flower.

common air. The latter specimen flowered very well; but the former died away without opening one of its blossoms.

With the same
in flower.

Exp. 2. The event was the same when I made use of a branch of this sedum already in flower; for the plant withered and did not shed its petals; which is contrary to the usual habits of this vegetable, when it is left to the impulses of nature unthwarted by art.

With barren
plants of the
same and sem-
pervivum tecto-
rum.

Exp. 3. Several plants of sedum acre were also confined in jars filled with azote last spring, before the flower buds were formed upon them: not one of these attempted to vegetate; on the contrary they all withered in the course of four or five weeks. This was not the case with other specimens, which were placed beside them upon similar stands, under jars containing common air and standing in water; for they remained green and vigorous, after the former were dead; they had moreover produced a number of purple filaments springing from the stalk; which is constantly done by the sedum acre, when suspended by the roots. It is proper to remark, that the air of the latter set was frequently changed, with a view to keep it near the standard of the atmosphere. The *sempervivum tectorum* did not offer to vegetate when treated in the same manner by being confined in azote; but it appeared to be more tenacious of life under this kind of treatment, than the sedum acre.

Inference from
these experi-
ments.

The preceding experiments induced me to conclude, at the time of making them, that the welfare of succulent plants requires them to be exposed to the atmosphere, and that they cannot grow when surrounded by azote, nor even preserve the vegetative principle in it, for an indefinite length of time.

Seeds and bulbs
in the same pre-
dicament.

I have shewn in the fourth volume of the Manchester Memoirs, that seeds and bulbs are not less indebted for their prosperity to the agency of oxygen, than succulent plants appear to be from the present letter. Thus it is manifest that the farther we carry our researches into the nature of organized bodies, the greater is the number of them; which are found to be incapable of performing their natural functions, when deprived of respirable air; but as the truth of this maxim remains to be extended experimentally to vegetables of a less humid constitution, than the sedums, the following fact may be related with propriety.

Experiment
made with
cerastium vulga-
tum,

Exp. 4. A diminutive plant of *cerastium vulgatum* was placed on the 18th of February, 1804, upon a stand covered with a

slice of wet sponge; it was then put under an inverted jar, containing common air, and standing in water. This plant encreased considerably in bulk in the course of 20 days; besides which it had taken root in the sponge; it was therefore transferred into a jar filled with azote; but this change of circumstances put a stop to vegetation; for the herb made no further advances, but withered away.

Exp. 5. In order to diversify these experiments as much as possible, I took a number of phials, having narrow necks, and filled them with water, which was either procured from snow, or deprived of air by boiling. Proper plants being then placed in the bottles, they were introduced quickly through water into jars filled with azote; under which circumstances, all my specimens died away sooner or later without making one effort to vegetate. The herbs used on these occasions were the common garden spear-mint and the draco-cephalum Moldavicum; both of which vegetate freely in bottles of water exposed to the atmosphere. I also treated in the same manner, a branch of *Lyfimachia vulgaris*, which was ready to flower; it declined gradually for a month, but never opened one blossom; though it is an aquatic plant and flowers very well in jars of water. It may here be remarked once for all, that vegetables die very slowly in azote, provided they are supplied with water deprived of air; but if my experiments may be relied on, all their natural functions cease in these circumstances, as soon as they are removed from the atmosphere.

Exp. 6. The loss of energy experienced by plants confined in mephitic air, does not appear to arise from an injurious quality of the gas, but from the want of the salutary stimulus of oxygen; for a slip of spear-mint, which had remained twelve days in a glass of azote, recovered upon being restored to the air, with the loss of its lowest leaves only; and an off-set of *sempervivum* vegetated freely after being removed from a jar of the same gas, in which I had kept it, from the 2nd of April to the 2nd of May, standing in a bottle of snow water. These facts seem to argue, that vegetables, treated in the manner described as above, suffer a species of torpidity, from which they may be recalled by the presence of atmospherical air, provided the re-admission of this necessary agent be not delayed too long; in which case, the plant perishes, in consequence of the

with plants in
water deprived
of air.

Plants die in
azote for want of
their proper
stimulus.

the continued inactivity of its organs, and the fermentation of its juices; which is occasioned by the suspension of all its functions.

Succulent plants absorb air from the atmosphere, but dry ones from water.

This fluid so necessary to the process of vegetation, is evidently absorbed from the atmosphere by succulent plants, bulbs, and a variety of seeds, when properly moistened; for they will all vegetate freely in open situations; but the following experiment seems to prove that trees, shrubs and the less juicy herbs derive air from water, as fishes do.

Exp. 7. I introduced the lower extremities of different plants, or more properly of the branches of different plants, into phials filled with water, and after luting the necks with wax, I sunk the bottles in pots of water, so as to leave the foliage of the plants exposed to the sun and surrounded by the atmosphere. The evaporation caused by the action of the light drew the water out of the bottles, and the air descending at the same time through the substance of the plants prevented a vacuum being formed in these close vessels. This fact had nearly persuaded me, that vegetables possess a faculty analogous to respiration; but when I repeated the experiment with phials perforated in the bottom, they remained full of water. This circumstance shews the respiration in question to be merely accidental and to be occasioned by the necks of the bottles being luted; on the other hand it is highly probable, that air enters the vessels of vegetables in conjunction with water. Here it is decomposed, in consequence of the stimulus given to the vegetative principle by its presence; the azote is afterwards assimilated into the substance of the plant, while the oxygen is rejected either wholly or in part. This gas, after its expulsion flows into the mephitic air, contained in the jars, the volume of which is thereby augmented, and its quality improved; but I will not venture to say, that the two gases form a new chemical compound on these occasions.

Exception to the general rule that plants require air.

Exp. 8. I am acquainted with a single exception to the general rule that air is requisite in the process of vegetation. For when plants have withered in azote they become mouldy; that is, in the language of botany, they are covered with the *mucor mucedo*. The worms which inhabit the decayed livers of sheep, &c. furnish us with a parallel instance in the animal kingdom; and the investigation of these singular anomalies forms a difficult problem in the natural history of organized bodies.

The sentiments contained in this letter, are opposite to the common notions relative to the nature of vegetation, and so manifest a departure from established opinions may be thought to require an apology. The justification of any conduct however appears in the body of the essay. The experiments are related with an attention to circumstances; which will enable almost any person to repeat them; and should some philosopher undertake to do it, his labours will either establish my opinions, or confirm the contrary doctrine, by the refutation of them. There is one omission in the detail of these experiments; I have neglected to remark, that the term azote signifies nothing more than common air deprived of oxygen by the sulphuret of potash or the martial paste of sulphur, but more frequently by the former. The determination of the question is of some moment, not only to natural history, but perhaps to agriculture also; and I could wish it to be resumed by a philosopher, whose advantages are superior to my contracted powers of observation and imperfect apparatus. Should any man of science undertake the task, I would recommend the sedum acre to his notice; because the filaments which spring from the stalk of this plant in common air afford a criterion whereby the progress of similar specimens confined in azote may be observed and ascertained.

JOHN GOUGH.

*Middleshaw,
October 21, 1804.*

SCIENTIFIC NEWS, ACCOUNT OF BOOKS, &c.

Frothing of Oil by Electricity.

THE anonymous author of some electrical experiments communicated to Van Mons the following fact, from whose Journal I translate it. He was making an experiment to ascertain the conducting quality of oil, for which purpose he had filled one third part of the length of a tube with the oil of Colza. The tube was three feet long and half an inch in diameter, having its closed end drawn out: The open extremity was secured by a cork, through which a needle was thrust; and over the cork was a coating of wax to prevent the oil from transuding. The tube

tube being then placed on two insulating supports, and the needle turned towards the part most remote from the machine, the tube was slightly inclined, in order that the oil might run down to that part of its capacity which was nearest the prime conductor. He then put the machine in action, and with great surprise observed the oil gradually to enter into a state of ebullition; beginning at the surface and speedily acquiring a state of effervescence perfectly resembling that of sparkling Champagne. Upon opening the tube, a gas escaped with violence, which the author did not collect, but which well deserves to be examined. This active electrician intends to subject the different vegetable and mineral acids to the action of electricity by the same process.

Native Magnesia.

GIOBERT has found that a white earth considered as pure alumine and employed at Turin in the fabrication of porcelain, contains 0,80 of magnesia. VAN MONS.

Gray's Experienced Millwright.

The Experienced Millwright; or a Treatise on the Construction of some of the most useful Machines, with the latest Improvements. To which is prefixed a short Account of the general Principles of Mechanics and of the Mechanical Powers. Illustrated with forty Engravings. By Andrew Gray, Millwright, very large quarto, 73 Pages of Letter Press, of the same Size as the Engravings.

THE author is a practical mechanic, who has been upwards of forty years employed in erecting different kinds of machinery; and the greatest part of the Descriptions in this Work are of Machines which he has either planned or superintended their construction. These Drawings being accurately made to scale, are not, as he remarks, to be considered as mechanical speculations, always doubtful till subjected to actual experiment, but as cases of practical knowledge, the effects of which have been fully tried and long approved; most of them being still employed for the purposes of their original fabrication.

Previous to his Account of the Machines, this author gives a popular Treatise on Mechanics, in six different chapters, beginning with General Principles and Definitions, which are followed

lowed by Explanations of Simple Engines, called the Mechanical Powers. These are succeeded by an Account of the Effects of Friction, and the best Method of applying those Mechanical Powers. Practical Directions for designing and constructing the Parts of Machines are in the next place given; and lastly, the author treats on the Strength of Machines, the relative Velocity of their Parts and the leading Doctrines which are necessary to be attended to in the erecting of Water-Mills.

The Machines of which Measured Plans, Elevations and Sections are given, are; A considerable number of Threshing Machines, to be drawn either by Water or by Horses, with the Elevation of a Wind-mill to turn Machinery of this Description; one Snuff Mill; two Sheeling Mills; several Corn, Barley, Malt, and Flour Mills; one Oil Mill; one Flax Mill; one Paper Mill; one Saw Mill; one Felling Mill; Bleaching Machines, Beetling Machines, and various Pumps.

These Drawings and Descriptions will no doubt prove of high value to the man of business; though it is perhaps to be regretted that the same attention to junior students which is displayed in the introductory treatise, is not as fully shewn by explaining the relative operations and effects of the parts of these machines in the descriptions which accompany the drawings.

Practical Observations concerning Sea Bathing; to which are added, Remarks on the Use of the Warm Bath. By A. P. Buchan, M. D. of the Royal College of Physicians. London. Crown 8vo. 207 Pages.

IN the early stages of human society, when the occupations of man led him to strong exercise in the open air, it is probable that his health may have been chiefly impaired by the fortuitous occurrences of his mode of life, and the direct wear of his organs engaged in the gross and active labours of his life. It is probable that he may have felt little of those diseases which the classification and habits of a refined and luxurious society have introduced. Neither mental fatigue, nor the anxious and incessant labour of crowded cities could have operated upon his powers, nor was he subjected to the miseries of indolence, repletion, and the listless want of motive which benumb the faculties of the rich. These are the prices which
by

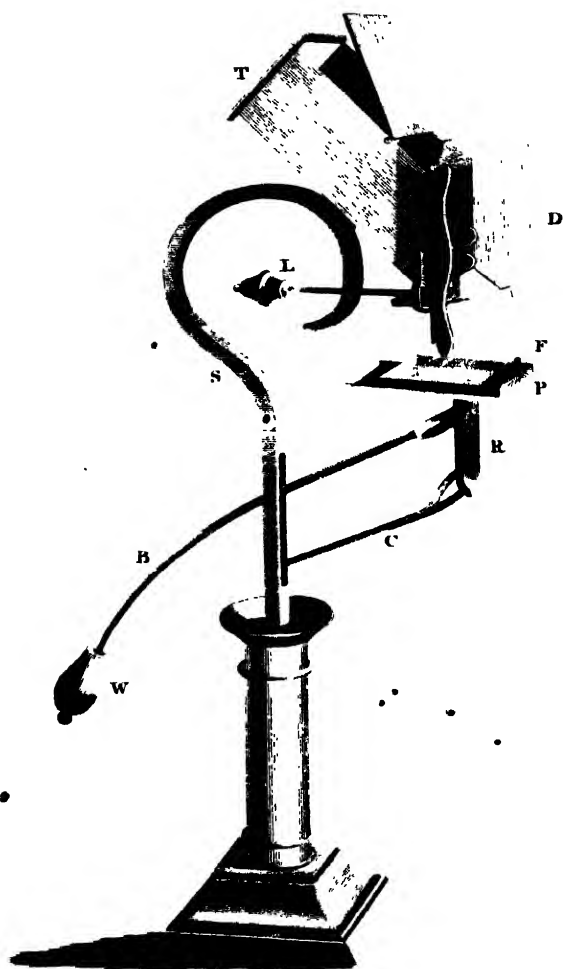
Buchan on sea
bathing.

Sea-bathing.

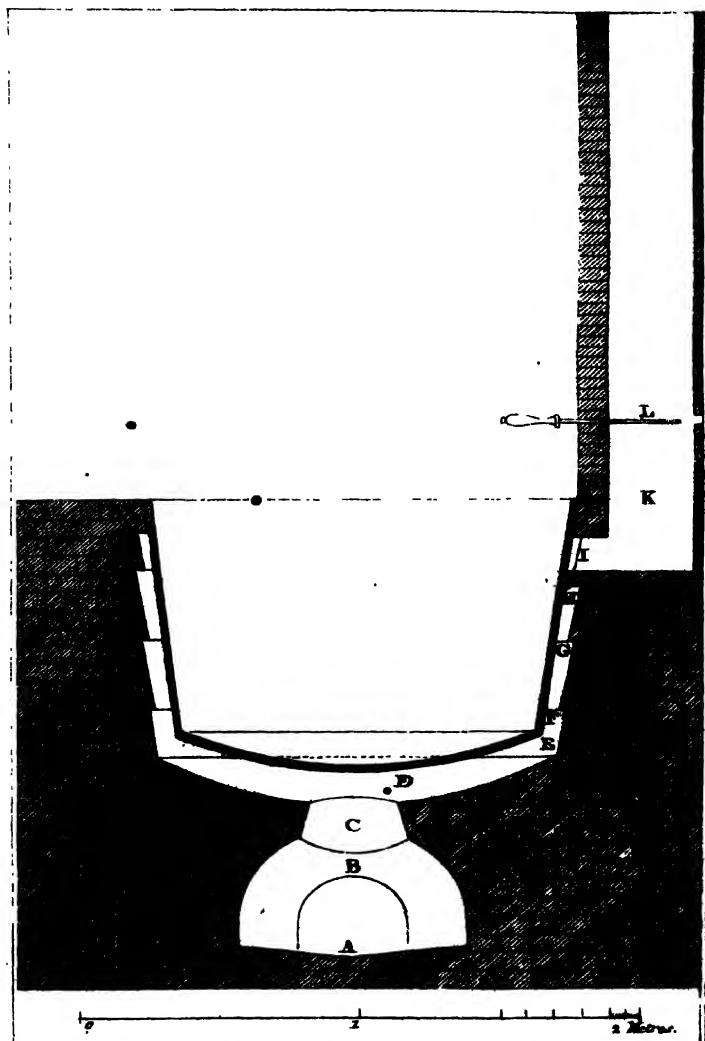
by our numerous mistakes we are compelled to pay for the superior enjoyments which an enlightened state of society can afford to those only who possess the virtue, the conduct, and the good fortune to know and to acquire them. Among the most oppressive of these consequences, no doubt, is the loss of health; and while we make sacrifices to what we imagine to be our welfare, it becomes us occasionally to interrupt our usual train of habits, to unbend the mind, give repose to the system, and recover, by an occasional change of our occupations, that vigour which sameness of pursuits and confinement seldom fail to impair.

One of the means at present much resorted to is, to repair to the sea-coast; partly with a view to enjoy a scene which, by its impressive magnitude and the numerous changes it undergoes, cannot but be highly interesting; but principally with the intention of bathing in the sea. Bathing is certainly of the greatest benefit in many cases, and, like every other practice capable of strongly affecting the human frame, it must also, in other cases be capable of operating to the injury of the health. We have no direct general treatise on this subject; and to give the requisite information to individuals by which they may safely and judiciously regulate themselves requires somewhat more than mere medical skill. The present work abounds with interesting facts and clear deductions, and is written with that candid spirit of philosophical remark which shews an intimate acquaintance with the moral as well as the physical conditions of our existence, and the accidents to which we are exposed. I am much mistaken if any one who begins to read this work will be disposed to quit it without perusing the whole; and there are few who can have so extensively considered the subject, or who are so little concerned in its discussion as not to derive advantage from the many points of information it contains.

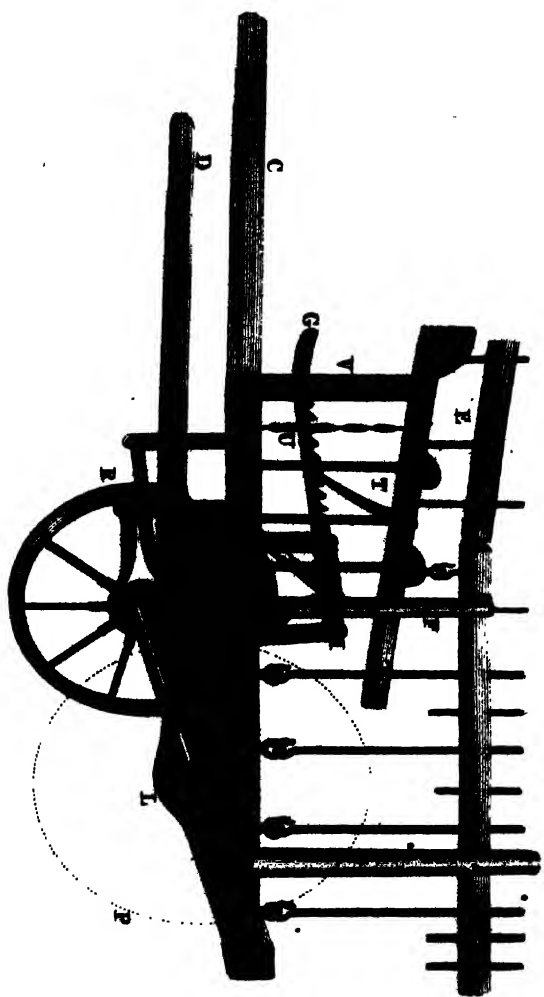
Teller-Lamp by W. J. H. Bearell.



Evaporating Furnace by Curraudan.



Mr. W. Burt's double Prince



Perpetual Motion.

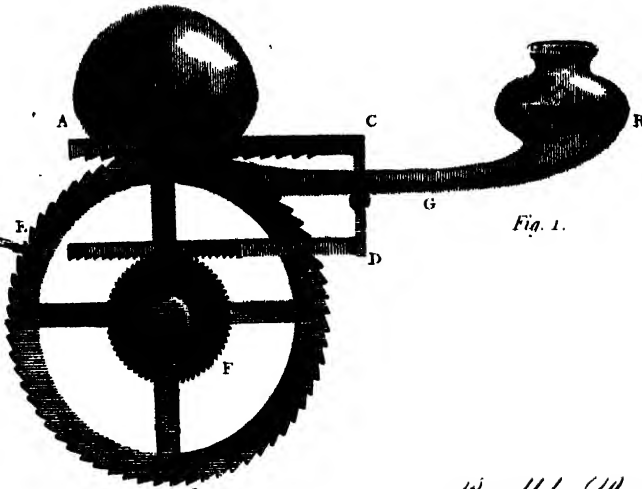


Fig. 1.

Parallel Ruler.

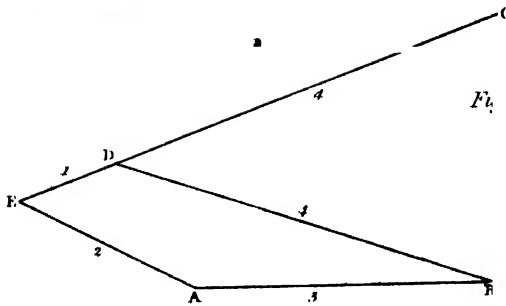
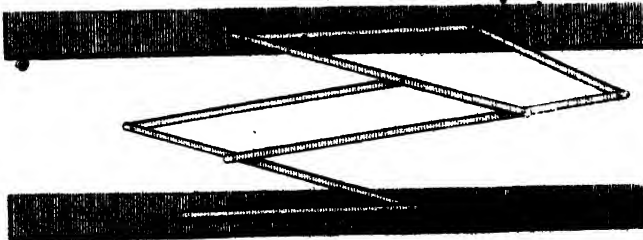


Fig. 2.

Fig. 3.



JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

DECEMBER, 1804.

"ARTICLE I.

Description of a Tubular Pendulum; having all the Properties of the Gridiron; but being more compact as well as more steady in its Motions. In a Letter from Mr. EDWARD TROUGHTON, the Inventor.

To Mr. NICHOLSON.

DEAR SIR,

EVER since the commencement of your Journal, I have often thought of registering therein, the improvements which I frequently make in astronomical and other instruments. No man perhaps had ever a greater antipathy to writing than myself, and drawn as I am into the vortex of business, and working with my own hands from nine to twelve hours in a day, leaves me but little time for literary pursuits; yet in one who values himself on account of the originality of his ideas, it must seem slothful not to commit them to the press, which until that is done, are like flowers or fruit growing fenceless by the way side, which every one that passes by may gather.

I always considered the pendulum of a clock more suitable to our stile of workmanship and habits of thinking, than those of any other description of artists; and agreeable to this idea, I have made pendulums near twenty years, and I own

Advantages of publishing improvements in philosophical apparatus.

Pendulums for clocks are particularly suited to mathematical workmen.

the subject is still agreeable to me, being yet unincumbered with patents, the reason of which no doubt is, that the few wanted holds out no prospect of gain.

Tubular pendulum.

Under these circumstances, I offer you the following description of my tubular pendulum, which I contrived about the middle of last July, and which I have just brought into action. The honourable mention which you made of my mercurial pendulum (*Phil. Journal* for May 1797) the excellent papers which you have at different times given to the public on this subject, and its importance to practical science, especially astronomy, leave me no room to doubt but this communication will be agreeable to you.

Stands on the principle of the gridiron.

It may here be proper to remark that the nature of this pendulum is by no means new; it is of the gridiron kind, and although extremely unlike it in outward appearance, is only a new species of that genus. Without further introduction, I will now proceed to give a brief description, with reference to the accompanying sketches, and then conclude with a few general remarks.

Description of the tubular pendulum by the drawing.

Fig. 1. and 2, Plate XIII. drawn to a scale of one eighth of the real dimensions, exhibit the shape of the whole instrument, in which the parts of action being completely concealed from view, it appears, excepting the usual suspension spring, to be made of solid brass. The first of these figures gives a front view, the latter shews it as seen edgewise. This figure of the bob is used more on account of its being easy to make, and lightly, than from any other considerations; it is made of one piece of brass, about 7 inches diameter, 2,5 thick at the centre, and weighs about 15 pounds avoirdupoise: the front and back surfaces are spherical, with a thick edge or cylindrical part between them. The apparent rod is a tube of brass, shewn in both figures to reach from the bob nearly to the top; this contains another tube and five wires in its belly, so disposed as to produce altogether (like the nine-bar gridiron of Harrison) three expansions of steel downwards, and two of brass upwards; whose lengths being inversely proportioned to their dilatation, when properly combined, destroy the whole effect that either metal would have singly. The small visible part of the rod near the top, is a brass tube, whose use is to cover the upper end of the middle wire, which is here single, and otherwise unsupported.

Reckoning from the top the first action is downwards, and consists of the spring, a short wire 0,2 diameter, and a long wire 0,1 diameter; these all of steel firmly connected, reach down within an inch of the centre of the bob, and occupy the middle line of the whole apparatus. To the lower end of the middle branch, is fastened the lower end of the interior brass tube, 0,6 in diameter, which terminates a little short of the top of the exterior tube, and produces the first dilatation upwards. From the top of the interior tube depend two wires 0,1 diameter, whose situation is in a line at right-angles to the swing of the pendulum, and reach somewhat lower than the attached tube itself, which they pass through without touching, and effect the second expansion downwards. The second action upwards is gained by the exterior tube, whose internal diameter just allows the interior tube to pass freely through it: its bottom is connected with the lower ends of the last described wires. To complete the correction, a second pair of wires of the same diameter as the former, and occupying a position at right angles to them, act downwards, reaching a little below the exterior tube, having also passed through the interior one without touching either. The lower ends of these wires are fastened to a short cylindrical piece of brass, of the same diameter as the exterior tube, to which the bob is suspended by its centre.

Fig. 3. is a full size section of the rod, in which the three concentric circles are designed to represent the two tubes, and the rectangular position of the two pair of wires round the middle one, are shown by the five small circles. By copying this arrangement, from the elegant construction of your own half seconds pendulum (*Philos. Journal for August, 1799*) I avoided much trouble, which must have occurred to me, unless indeed, I had been impelled on the same idea, by the difficulty of contriving the five wires to act all in a row, with sufficient freedom and in so small a space. *Fig. 4.* explains the part which closes the upper end of the interior tube: the two small circles are the two wires which depend from it, and the three large circles shew the holes in it, through which the middle and other pair of wires pass.

Fig. 5. is designed to explain the part which stops up the bottom of the interior tube, the small circle in the centre is where the middle wire is fastened to it, the others the holes for the other four wires to pass through. *Fig. 6.* is the part which

Description of
the tubular
pendulum by
the drawing.

closes the upper end of the external tube, the large circle in the centre is the place where the brads covering for the upper part of the middle wire is inserted, and the two small circles denote the fastening for the wires of the last expansion. *Fig. 7* represents the bottom of the exterior tube, in which the small circles shew the fastening places for the wires of the second expansion, and the larger ones the holes for the other pair of wires to pass through. *Fig. 8.* is a cylindrical piece of brads, which shews how the lower ends of the wires of the last expansion are fastened to it, and the hole in the middle is that whereby it is pinned to the centre of the bob. The fastening of the upper ends of the two pair of wires is done by screwing them into the pieces which stop up the ends of the tubes, but at the lower ends, they are all fixed as represented in *Fig. 8.* I have only to add to this description, that the pieces represented by *Fig. 7* and *8*, have each a jointed motion, by means of which the fellow wires of each pair would be equally stretched, although they were not exactly of the same length.

In the apparatus thus connected, the middle wire will be stretched by the weight of the whole; the interior tube will support at its top the whole except the middle wire: the second pair of wires will be stretched by all except the middle wire and interior tube; the exterior tube supports at its top the weight of the second pair of wires and the bob, and the second pair of wires are stretched by the weight of the bob only.

The first pendulum which I made of the tubular kind, had only three steel wires, and one tube above the bob; that is two expansions down and one up; and the quantity which one of brads falls short to correct two of steel, was compensated for, by extending those branches of the rod below the bob, and bringing up an external tube to which the bob was affixed. There is an awkwardness in this construction, owing to the rod reaching about 13 inches below the lower edge of the bob, otherwise, it is not inferior to the one first described.

Difficulty of
preventing flexure
and starting
motion in the
gridiron,

The principles of the gridiron pendulum, I believe, have never been questioned, indeed they cannot be; the difficulty of constructing it strong enough to prevent lateral flexure in the lifting bars, and consequent friction in the holes of the different crossing pieces necessary to bind it together, which occasion it to act by starts, has been the only obstacle to prevent its general application to the best clocks.

To

To remedy this evil I contrived the one here treated of, and ^{—is prevented by the use of tubes.} in my own judgment, have succeeded as near perfection as any effort of mine ever did. The tubes are drawn straight and regular in an engine, in the manner that wire is done: and their strength assure success, preventing the possibility of any sensible bending, while the freedom of action prevents any irregular motion whatever.

I have no farther quarreled with my own construction of the mercurial pendulum, the principles of which are faultless, and ^{mercurial pendulum is perfect but not portable.} form elegant; than that it is quite unfit for carriage, and an article too hazardous for me to deal in; but after all, candour obliges me to give the preference to the original one of Graham, which on account of some senseless criticisms about one part taking heat sooner than another, evaporation of the mercury, &c. was too hastily laid aside. Graham undoubtedly ^{Graham's pendulum approved.} made his pendulum before the expansions of either brass or mercury were accurately known; but an ingenious friend of mine, has from the best experiments, computed the different parts to mathematical precision, and introduced it a second time to the world; and already, there are not wanting proofs of its practical excellence.

Smeaton's pendulum with tubular bob is a good one, but ^{good; but not portable.} on account of the glass rod, like my mercurial one, is totally unfit for transportation, and like Graham's, is unsightly; while all three labour under the unavoidable disadvantage of not being applicable to the pyrometer in their compound and pendant state.

I am about to construct a pyrometer fit to try the tubular pendulum in its finished state; a pin fixed in the centre of oscillation, shall be contrived to act on a nice spirit level, and shew the expansion, if any, in angular measure: by which I shall be enabled to adjust it; for I have no idea of adjusting one by the going of a clock, that complicated apparatus will always, I fear, have too many errors of its own, to be used as a criterion of the merits of this more perfect instrument.

I will here subjoin the lengths of the different parts of tubular pendulums in inches; also, the expansions of brass and ^{Dimensions and grounds of construction of the tubular pendulum.} steel from which I computed them. The length of the interior tube is 31,9, and that of the exterior one 32,8, to which must be added 0,4 the quantity which in this pendulum, the centre of oscillation is higher than the centre of the bob; these

Expansions of
brass and steel.

- all of brass. The parts which are of steel are the middle wire, including 0,6 the length of the suspension spring, is 39,3, first pair of wires 32,5, and the second pair 33,2. From experiments which I tried in the year 1794, I found my brass to expand with 60° of heat 0,000640 on an inch, and my steel 0,0003966. Smeaton obtained under the same circumstances 0,0006444 for brass, and 0,0003833 for steel. Roy found brass to expand 0,0006316 and steel 0,0003816.

The above lengths computed from my expansions will I think be found very correct; if they are tried by Smeaton's, the pendulum will seem to err in excess, or gain with heat, about 6",9 a day with 60° of Fahrenheit; if Roy's be used, it will err also in excess, under the same circumstances something more than one second. The above statement shews with what care experiments of this kind ought to be conducted. All the dimensions are given in inches and decimals.

If my pyrometrical experiments turn out successful, they may likely be the subject of another communication; and I will reserve till then my reasons for giving the preference to my own expansions in the construction of the instrument here described; for this letter, being twice as long as I first intended, shall now be concluded by,

DEAR SIR,

Your obliged and obedient Servant,
EDWARD TROUGHTON,

November 3, 1804.

II.

Letter from W. JESSOP, Esq. on an Improvement in the Process of blasting Rocks with Gun-powder.

To Mr. NICHOLSON.

SIR,

IF I am communicating to you what you already know, you will accept as an apology for it that it is not generally known, and will be interesting to all who are concerned in mining or blasting rocks with gunpowder.

Usual process of
boring and
blasting.

The usual process, after drilling a hole and charging it with powder, is to introduce a wire or small iron rod, to preserve a com-

communication with the fuze, and then to ram up the remainder of the hole with stone pulverized by the operation of ramming it; after which the wire is withdrawn and the priming introduced.

This is a tedious operation, often attended with danger, and frequently the labour and powder is lost by the priming hole being obstructed in striking out the wire with a hammer. is tedious, dangerous and uncertain.

I had been informed that instead of this tedious operation, the blasting had been effected by introducing a straw filled with fine powder, and then filling the hole with loose sand. Loose sand proposed instead of ramming.

I hardly could believe this, conceiving that the part where there would be the least resistance would first yield to the explosion, and therefore that the loose sand must of course be blown out.

But in the month of August last I tried the experiment in some very hard rock at Fortwilliam and it completely succeeded: I since then have tried it on the lime rocks at Bristol with the same effect. Success of the experiment.

A few days since, wishing to know how small a quantity of sand would produce the effect, I caused a hole of $1\frac{1}{2}$ inch in diameter and 12 inches deep, to be bored in a knotty piece of oak of about 20 inches in diameter and charged it with three inches of powder, and upon that four inches of sand, intending (as I supposed that must be blown out) to repeat the quantity of sand inch by inch to the quantity that would be sufficient, but to my great surprize, it split the piece with great violence into six pieces. Oak 20 inches diameter split in pieces under a charge of 3 inches sand.

I then repeated it on a similar log of oak, with a charge of two inches of powder, and three inches of sand; it split the piece in two, and sent half of it into the air to the distance of forty yards. The same effect with less powder.

I think it may puzzle a philosopher to account for this: I have supposed that the sand in contact with the powder, first moving, the particles wedge each other fast before the motion has time to be communicated to the whole. Conjecture as to the cause.

I have reason for thinking that a much lesser quantity of powder will produce the effect in this way than by the common method; for as it lies loose in its chamber and makes itself more room by a little yielding of the sand, the whole will take fire, and especially if when half the charge is put in, the straw be introduced and then the other half charge, so that it may take fire in the middle of the charge. Less powder will probably be required in this way.

That

The sand appears to yield a little.

That the sand will yield a little, I think I may infer from having charged a tube of tin 14 inches in length and 1½ diameter, with three inches of powder; on exploding it immersed in water, I found *five inches* of the tube abruptly cut off, while the remainder was uninjured, but I do not consider this inference as decisive.

For want of time I was prevented from trying how much *less* than three inches of sand would have been sufficient.

I am, Sir,

Your most obedient, —

W. JESSOP.

London Coffee House,

Nov. 8, 1804.

A charge of sand would probably destroy artillery.

P. S. Might not this be useful on the evacuation of a fort, in destroying cannon, instead of the ineffectual mode by spiking?

III.

*On the Mucilaginous Matter of certain Vegetables, and their Use as a Substitute for Gum Arabic: by Mr. THOMAS WILLIS; being a Continuation of Experiments made upon the Subject by him, in Addition to those formerly published in the Transactions of the Society of Arts.**

Letter to CHARLES TAYLOR, Esq.

SIR,

Mucilaginous powders from vegetable roots.

I HAVE taken the liberty of presenting to the Society for the Encouragement of Arts, &c. specimens of the powder of vernal squills, of white lily-roots, and of salop, for their consideration and trial. I believe the bulbs of the vernal squill^o will be found to be equally useful as those of the hyacinthus non-scriptus; and the strong mucilage from the white lily-root and the salop much more so.

As gum-arabic has been at times, during the last war, above 30l. per cwt. the multiplication of different substances that will answer the same purpose, will assuredly be an object of a very interesting nature to manufacturers in time of war.

* Extracted from their Transactions, Vol. XXI. The former experiments are in our Journal, VII. 36.

I have

I have sent all the powder of white lily-root I had left; but am at the present time drying some taken from the garden two or three days ago. As soon as it is powdered, I will send you a large quantity of it.

I am, Sir,

Your most obedient Servant,
THOMAS WILLIS.

March 24, 1803.

SIR,

FROM the candour and approbation my paper on the use of the hyacinthus non-scriptus, as a substitute for gum-arabic, in some trials in calico-printing, met with from the Society for the Encouragement of Arts, &c. I resolved to pursue the subject further; and now offer the following observations for your consideration, in hope that trials may be made, that will be found to be of general benefit.

Having frequently observed, in old gardens, that the vernal squill grew very prolific, I conceived the root of them might be equally as mucilaginous as the roots of the blue bells. I procured three pounds six ounces of them on the 8th of July, 1802, and sliced and dried them. They produced one pound one ounce of powder, one drachm of which was dissolved in four ounces of water, by letting the mixture boil a minute or two. When the liquor was cold, there was a mucilage full as strong as one made with a drachm of the powder of blue bells, in the same proportion of water; from which circumstance, I should think the vernal squill will answer the same purposes as the hyacinthus non-scriptus. If it should, it is a root that may be very easily and plentifully propagated, and whenever gum-arabic is dear, may be found useful. It is to be remarked, that I found no pungency in the powder of the vernal squill. I have frequently eaten of it, and the taste is rather agreeable.

Root of the
vernal squill very
mucilaginous.

On the 18th of August, 1802, I collected four pounds of the white lily root, which yielded, when dried, rather more than one pound of powder. A drachm of it was dissolved in four ounces of water, by gently boiling it a minute or two: the mucilage was much stronger than that made with the vernal squill, but somewhat darker coloured. This root may possibly

White lily root
also very mucilaginous.

possibly answer the same purposes better than those above mentioned. The powder of the lily-root leaves a bitterness on the tongue.

Gum obtained
by solution and
evaporation.

Twelve ounces of the fresh roots of the white lily being bruised and pressed, yielded by evaporation one ounce and one quarter of brown gum.

There being a small quantity of it, I made no trial with it; but very probably the expressed juice might be used by the calico-printers with advantage. These roots can be procured at all times, and propagated to any extent; but it must be observed, that these bulbous roots are stronger when they are without stems, or only beginning to shoot out leaves; and the present time is as proper as any that can be for procuring them.

Comfrey root.

I tried comfrey-root; but the dark colour of the cuticle of the root affected the solution, by making it of a dark dirty brown.

I do not pretend to claim any merit in making the above experiments. Every one who is acquainted with these roots, well knows that they are all mucilaginous. My design is only your patronage, to render them useful in the arts, that their virtues and effects might not lay dormant, but be rendered serviceable to trade.

Powder of
salop root affords
very strong
mucilage; prob-
ably cheaper
than gum-arabic.

After making the above trials, the powder of salop-root was used, by dissolving one drachm of it in four ounces of water, in the manner above mentioned. It produced a very strong mucilage, and, when cold, was a perfect jelly, and much clearer than either of the other solutions. I am greatly of opinion, that the powder of this root will not only answer all the purposes of gum-arabic, but will be found full as cheap, if not cheaper, in proportion to its strength, than gum-arabic; but this is submitted to your consideration and trial.

I have sent some specimens of the Powder of the Vernal Squill, of White Lily Root, and also of the Salop Powder, in order that they may be tried by your direction; and if they should be found of benefit to commerce, nothing would give me greater pleasure, than to find my slender abilities have been of utility to my country, and honoured with the approbation of the Society.

I am, Sir,

Your obedient servant,

THOMAS WILLIS.

March 22, 1803.

Examination

IV.

Examination of Mr. Ezekiel Walker's Experiments and Theory of the Enlargement of the Horizontal Moon. In a Letter from C. L.

To Mr. NICHOLSON.

SIR,

YOUR correspondent Mr. Ezekiel Walker, has favoured the world with a new attempt to account for the apparent enlargement of the horizontal moon *, in which, after indulging his wit, and expressing his astonishment at the expence of those who have laboured on the same subject, he proceeds to assure us that the *pictures on the retina are not permanent but vary as the dimensions of the pupil vary.*

That you, Mr. Nicholson, who are in some respect accountable to the world for what you admit into your Journal, and who possess a just celebrity for the manner in which you have executed the task,—that you should have omitted to favour the writer and your readers so far as to reject his paper, is to me no otherwise to be explained than by referring it to your impartiality; which may have induced you to leave the refutation of alleged facts to your correspondents rather than enter into a discussion respecting them yourself.

I cannot for a moment suppose you to have had a doubt concerning this imaginary novelty in optics. If Mr. Walker's position were true, the magnifying powers of the same telescope would vary with its aperture; a well illuminated theatre or room would become contracted in its apparent dimensions; the paper at which I now look would suddenly become larger when I cover the candle with my hand; and in a word we should have no certainty if the magnitudes of things were to appear different with every change of the pupil.

None of these things do in fact happen, and it seems almost trifling to insist upon them. Still less is it necessary to have recourse to lines and figures: but for the sake of your less instructed readers I will meet Mr. Walker by correct experiments of the same description as his own.

* Philos. Journal, IX. 164.

The image formed by a lens with a broad aperture is the same as

Exp. 1. I took a clear double convex lens supported on a stand, and placed the same opposite an Argand's lamp; so that a good image of the flame was formed upon a paper fixed against the wall of the room. From the flame to the lens the distance was $49\frac{1}{2}$ inches and from the lens to the image it was 88 inches. The horizontal breadth of the image was repeatedly measured = 1.58 inches and to a greater degree of precision than one hundredth of an inch.

with a contracted aperture.

Exp. 2. The lamp and lens were suffered to remain unaltered in the situations they possessed in the former experiment; but a screen was placed close to the lens, having a circular hole of one inch diameter concentric with the lens itself. The image was then fainter but very distinct, and measured exactly the same as before.

In both these experiments the papers on the wall were several times changed to prevent any deception from the appearance of former measures; and the measures were taken along the same horizontal line by the help of a line on the wall.

Hence it was seen that a diminution of the pupil or aperture, to admit less than one thirty-eighth part of the light, made no difference in the image.

The experiments repeated with an achromatic lens.

Exp. 3. I took a good achromatic object glass of Dollond, having an aperture of 1.9 inch and placed it at the distance of $52\frac{1}{2}$ inches from the lamp; when it gave a good image on the wall at the distance of $70\frac{1}{4}$ inches from the lens. The horizontal breadth of the image was 1.2 inches.

Exp. 4. When the experiment was repeated with no other change than that a screen with a central hole of 0.3 inches was placed before the lens, the image proved 1.2 inches as before.

The two last experiments were made with the same care and precautions as the former. They shew that no diminution of the image takes place when the pupil is diminished to admit less than one fortieth part of the light.

Probability that Mr. W. was deceived, and how.

I may not conclude, Sir, without making some observations on the direct contradiction of my experimental results to those of Mr. Walker's. May I ascribe to him any thing more than inaccuracy? Assuredly I do not. Nevertheless, if he measured so uncertain a thing as the length of the flame of a candle without being assured by repeated experiments that its permanency was entitled to some confidence; if he held his screen

nearest

nearer to his lens when the light was rendered fainter by his card; if he mistook the greater visibility of the smaller image for a proof that it was in the focus of the lens; in a word, if he did not pause and examine former facts before he adopted a conclusion so repugnant to many of them, I would submit to yourself and readers whether he has acted consistently with the rules of philosophical investigation, or has any reason to prize his own solution beyond that which is founded on the rules of linear perspective*.

I am, Sir,

Your humble servant,

C. L.

V.

The Method of preparing Chinese Soy. By MICHAEL DE GRUBBENS.†

IN the acts of the Swedish Academy, for 1764, page 38, we find a description of the preparation of Chinese Soy, by the late Captain Ekeberg; but as this description is not only incomplete but even deficient in accuracy, so as not to produce the true Chinese compound, I am well convinced that Mr. Ekeberg must have been unacquainted with the actual process. It is most likely that he depended upon information given him by the Chinese who are not always to be trusted; a fact of which I have had sufficient reasons to be convinced, during

Ekeberg's account of soy is erroneous.

* Mr. W. having supposed that the want of land objects must take away the notion of distance at sea, reminds me of an incident concerning the Panorama of Black-Friar's Bridge. This picture was exhibited with indications of a considerable wind with waves on the river, at the same time that the buildings on the London side were seen reflected in the water. I took notice of this inconsistency, and the ingenious author replied that the water had been at first painted as if smooth; but it was found necessary to put in the waves in order to give distance by their regular diminution in perspective, and that the reflections were kept in from a notion that they would rather add to the effect than offend by their want of perfect truth.

† Extracted from the Memoirs of the Academy of Sciences of Stockholm for 1803, by M. Linbom, and inserted in the *Annales de Chimie*, Vol. L. from which the present translation is made.

my

my five years residence in their country, in my attempts to ascertain correctly the manner of treating a species of silk-worm which affords its thread five or six times in the year, as well as their methods of dying cotton and silk and several other branches of domestic œconomy.

Having since received for a considerable remuneration true notions concerning these objects, I saw how far the former recitals had differed from the truth. The same thing happened in my attempts to discover the preparation of soy; but as I at last became perfectly acquainted with it, I think it proper to communicate the same to the academy.

Ingredients of
soy.

Soy is prepared with a species of haricots (which are white and smaller than those of Turkey) wheat flour, common salt, and water. The following are the proportions; 50 pounds of haricots, 50 pounds of salt, 60 pounds of wheat flour, and 250 pounds of water.

Process.
Haricots are
boiled in water,

After having well washed the haricots, they are boiled in well-water in an open vessel for some hours, or until they have become so soft as to be worked between the fingers. During the boiling they must be kept covered with water to prevent their burning, and care must be taken not to boil them too much, because in that case too much of their substance would remain in the water of decoction. The haricots being thus boiled, are taken out, and put into large shallow wooden vessels, which in China are made of thin staves of bamboo, two inches and a half in depth, and five feet in diameter. In these they are spread out to the depth of two inches, and when they are cold enough to be worked with the hand, the wheat flour is gradually thrown in and well mixed with the haricots, until the whole of the before-mentioned quantity has been used. When the mass becomes too dry, so that the flour does not mix well with the haricots, a little of the hot water of decoction is added.

—then mixed
with wheat
flour,

—and thinly
spread out in a
vessel covered
up.

A peculiar
mouldiness is
suffered to come
on by admitting
the air :

The whole being well mixed, the mass is then spread abroad in the vessels before-mentioned, taking care that its depth shall not be more than an inch or an inch and a half; and the mass is then covered by a lid which fits exactly. When the mass begins to become mouldy and heat is disengaged, which happens after two or three days, the cover is then raised by putting two sticks beneath it, in order that the air may have free access. During this time a rancid odour exhales, and if the
mass

mafs becomes green, it is a sign that the whole goes on properly; but if it begins to be black, which must be carefully noticed, the lid must be elevated still more, in order that the mafs may have more air. If it once becomes black, the whole is spoiled.

As soon as all the surface is covered with green mouldiness, which usually happens in eight or ten days, the cover is taken off, and the compound is exposed to the sun, and the air for several days. When it has become as hard as a stone, it is cut into small fragments, which are thrown into an earthen vessel, upon which the 250lbs. of water, having the 50lbs. of salt first dissolved in it are poured. The whole is then well stirred together, and notice is taken of the height at which the water stands. If it be not convenient to put all the mixture into one vessel, a number may be used, taking care that the materials be proportionally distributed in each. The vessel thus filled is placed in the sun, and its contents stirred up regularly every morning and evening, and a cover is put on at night to defend it from the cold, as well as to prevent any rain from finding entrance either by day or night. The hotter the sun the sooner will the soy be completed. The process is seldom undertaken but in the summer, notwithstanding which it lasts two or three months.

—the compound is then dried,

cut in pieces and mixed with salt water :

and in a vessel placed in the sun and daily agitated

for two or three months.

As the mafs diminishes by evaporation, well-water is added; and this digestion is continued till the salt water has entirely dissolved the flour and the haricots. The vessel is still left for some days in the sun, in order to complete the solution still more effectually, as the good quality of the soy depends upon this circumstance; and the daily stirring or agitation is continued to the very last.

When the fluid has become fine and homogeneous

When at length the mafs has become very succulent and oily, the whole, as well the thick as the more fluid portions, is pushed into bags, through which the soy is pressed, and is then clear and ready for use. It is not afterwards boiled, as Mr. Ekeberg pretends. It is to be kept in bottles well corked. The Chinese who deal in this article keep it in large pitchers well closed. Before it is strained in the press the soy is of a deep brown colour, but it afterwards becomes black.

it is strained by pressure and is soy.

The Chinese also prepare two kinds of soy from the dregs which remain. The first time they add 150lbs. of water and 30lbs.

Inferior soy from the dregs.

30lbs. of salt, and after having pressed the mass, they again add 100lbs. of water and 20lbs. of salt, always proceeding as before described.

The two last kinds of soy are not strong, but very salt, more especially the latter, which is also lighter coloured. These two kinds are the most common in China, and are used both by natives and Europeans. The differences of price are as 8.4.1.

This process has been often verified,

In this manner I prepared in 1759, at my residence in Canton, the whole of the soy I made use of, and I also brought several bottles with me to Sweden. It was succulent, oily, moderately salt, and very different from what is commonly sold in Europe. With regard to its taste, it might be put in competition with that of Japan, which is generally considered as the best.

by the writer himself.

This description is more particularly to be depended upon, as I always made the preparation myself; and I can venture to assert that this process is the only one for obtaining soy of the best quality.

Soy contains no spice or sugar, and is sold in China for three half pence a pound.

Mr. Ekeberg affirms that the soy is boiled with the addition of sugar, ginger and other spices; but this is without foundation, and cannot be true, for a lb. Chinese of soy costs no more than two candareens of Chinese money*. This was the common price during my whole residence at China, and is too low to admit of these ingredients in the preparation. It is also the fact that soy has no indication either of sugar or spice in its taste; its principal character is saline.

VI.

On the Laws of Galvanism. In Letters from C. WILKINSON and THOMAS HARRISON, Esqrs.

Windermere Lake, Westmoreland, Nov. 8.

DEAR SIR,

Some account of Mr. Gough.

I TAKE the opportunity of a few days relaxation in this romantic and picturesque situation, to trouble you with some

* The Chinese pound or tael is, I think, about 20 ounces, and the candareen is a little more than three farthings, that is to say, it is one tenth part of the mace which is valued at eight-pence. N.

galvanic

galvaanic observations, which principally originate from a very pleasurable conversation I have lately had with my friend Mr. J. Gough, of Kendal. To you, Sir, this gentleman, as a philosopher and mathematician, is well known. Although deprived of sight in very early infancy, it is amazing to observe the rapidity with which he proceeds in geometrical investigations. Independent of this very striking acquisition, he is the best botanist and natural historian this country possesses. To these branches of knowledge he adds very considerable classical knowledge; and in his pneumatic enquiries he places no dependance upon any of the gases which he does not prepare himself. To converse with such a person is to be improved. To some lectures I delivered on galvanism a few days ago, at Kendal, I was honoured with his attendance; and in consequence of the law I was attempting to explain, relative to the powers of galvanism on metallic bodies, I was favoured, the subsequent day, with the following letter from a very intelligent and well-informed professional gentleman at Kendal, Mr. Harrison, brother-in-law to Mr. Gough; the particulars of which I dare say you will find sufficiently interesting to merit insertion in your valuable Journal.

TO C. WILKINSON.

DEAR SIR,

Kendal, Nov, 5, 1804.

I MENTIONED to you yesterday my doubts respecting the law of galvanism, which you have laid down in Nicholson's Journal for March 1, 1804; where you say, that the igniting powers in batteries of the same total surface, are as the squares of the surfaces of the elementary plates, singly taken in each: Investigation of the power of different piles in burning wire.

• This, if I mistake not, you repeated in your lecture of last night; at which time I expressed my doubts to Mr. Gough, and we afterwards examined the data given in the paper before-mentioned; on which we made the following calculations, and found that your conclusion was strictly true according to your data, but that the forces of different batteries exposing unequal surfaces, will be in the ratio compounded of the number of plates in each battery, and the sixth power of the sides of the elementary plates singly taken in each. The data you have given are the following: 100 plates of four in. sq. ig-

Investigation of
the power of
different piles in
burning wire.

nited half an inch of wire; 400 plates ignited two inches; and 50 plates of eight inch. sq. ignited 16 inches of the same wire.

Now in order to accommodate these numbers to algebraic calculation, we will suppose a comparison of three batteries, two of which are composed of plates of equal diameters, but differing in number; the third shall also differ from the first and second in the magnitude of its plates, and agree with the second in the number of them.

Let n be the number of plates in the first, m the number in the second and third batteries; w and y the diameters of the two sets of plates. Then, if the power of any single plate bear any constant ratio to its surface, it will also have a constant ratio to some power of its diameter, because the surface is as the square of the diameter in similar figures: let p be that power; let l , k , and z express the lengths of wire which are ignited by the 1st, 2d, and 3d batteries; then from the first datum, $n : m :: l : k$, because the effect is as the number of equal plates, according to experiment. Again, seeing that w is the diameter of the plates in the first and second batteries, and y the diameter of those composing the third, and p expresses the power determining the law of force for plates of different diameters, the force of a plate in the first and second batteries, will be to the force of a plate in the third, as $w^p : y^p$, and as the number of plates in the second and third is

Plate. Plates.

equal, we shall have $mw^p : my^p :: k : z$, because as $1 : m ::$

Plate. Plates.

$w^p : k$, and as $1 : m :: y^p : z$, then as $w^p : y^p :: k : z$, but as $n : m :: l : k$; therefore $nw^p : my^p :: lk : kz :: l : z = \frac{lm y^p}{n w^p}$.

To apply this general theorem in order to determine the value of p in all cases, we will begin with your own experiment made with equal surfaces on different numbers.

Since 100 plates, of four inches, ignite half an inch of wire, and 50 plates, of eight inches sq. ignite 16 inches, we have $n = 100$, $m = 50$, $w = 4$, $y = 8$, $l = \frac{1}{2}$, $z = 16$; therefore $100 \times 4^p : 50 \times 8^p :: \frac{1}{2} : 16$; but since $8 = 2 \times 4$, $8^p = 2^p \times 4^p$, therefore $2 : 2^p :: \frac{1}{2} : 16$, and $16 = \frac{2^p}{2^2} = 2^{p-2}$;

but

but $16 = 2^4$; hence $2^4 = 2^p - 2$; therefore $p = 6$; hence the force of plates are as the 6th power of their diameters.

When the surfaces are equal, the igniting force is as the 4th power of the sides of the individual plates; for in this case we have $n w^2 = m y^2$, and $n : m :: y^2 : w^2$; therefore the theorem $n w^6 : m y^6 :: l : z$, becomes $y^2 w^6 : w^2 y^6 :: l : z$; that is, $w^4 : y^4 :: l : z$, which is the law in your particular case.

This theorem will be found useful in estimating the force of batteries in general; as an example of which we shall calculate, by means of it, the force of your intended battery, consisting of 50 plates, each 24 inches diameter. In order to determine the quantity of wire that will be ignited by such a battery, we have $n = 100$, $m = 50$, $w = 4$, $y = 24$, or six times w , $l = \frac{1}{2}$; therefore $100 \times 4^6 : 50 \times 6^6 w^6 :: \frac{1}{2} : z$; that is, $2 : 6^6 :: \frac{1}{2} : z$, and $z = \frac{6^6}{4} = 36^3 \times 9$ inches, or 972 feet.

I am, Dear Sir,

Your's respectfully,

THOMAS HARRISON.

Stramongate, Kendal.

YOU will observe, Mr. Editor, from the above theorem, ^{experiments on} that my statement of the powers of two different galvanic ^{the decomposition} arrangements, equal in their metallic surfaces, being as the squares of the surface of two individual plates, or, which is the same, as the 4th power of the diameter of each plate, is correct only where the metallic surfaces are equal; when they are not equal, then the powers are as the 6th power of the diameters, or as the cubes of their respective surfaces.

In a short time you will have the pleasure of a paper from Mr. Gough, relative to the decomposition of water by galvanism: With this view we tried together the following experiments, the results of which tend to favour the explanation I have attempted in the Elements I sometime since published.

For this purpose I employed 270 plates, containing a surface of 6720 square inches, and connected with the positive and negative sides by wires of platinum. These platinum wires were inserted in a dish of water at eight inches from each other; the positive wire disengaged oxygen gas, the negative

wire hydrogen gas. The decomposition took place very slowly; in proportion as the wires were approximated, the decomposition increased in the same ratio.

The platinum wires were then inserted into a capillary tube whose bore is about $\frac{1}{32}$ of an inch, filled with water; no sensible effect was produced till the wires were placed at the distance of half an inch, and then only a solitary bubble became disengaged from the negative wire, never completely detached, but hovering round the extremity, not being set at liberty by the evolution of another particle.

When the platinum wires were inserted in a tube whose bore is $\frac{1}{8}$ of an inch, at the distance of six inches a slight decomposition was observed, and which necessarily increased in proportion as the wires approached each other.

When the platinum wires were inserted in different glasses of water, and the extremities of a frog deprived of its integument constituted the connecting medium between the two glasses, or a wet piece of cloth, at the distance of three inches a slight decomposition was observed.

When two wires are connected with the different ends of a galvanic battery, the wires immediately participate, as to their electrical intensity, with the respective plates with which they are in contact.

Investigation of the laws of decomposition of water by galvanism.

Let us suppose P the wire from the positive end of the battery, and N the negative. *Pl. XII. Fig. 2.* Let us suppose that the galvanic arrangement is such, that, when the wires are placed at the distance a, a , the decomposition commences. If the wires be placed at b and b , the decomposition is considerably increased; and this increase will go on, till the maximum will be at d, d , when the wires are infinitely near each other without being in contact.

The positive wire, according to its electrical intensity, produces a corresponding state in all fluids with which it is in contact, proportionate to their capacities; so also the negative wire; and it is evident from the principle of electrical charges, that the influence of each must extend to equal distances; it will therefore follow, that when at a, a , supposing these points the apices of two equal cones, the areas of their respective bases will be the least possible, and consequently, in their concurrence at i , will only influence a single particle of water. When the wires are placed at b, b , then $c i$ will express

pres; the radius of the circular base (supposing $bc = ai$), and consequently a greater proportion of water subjected to the influence; and therefore evidently when at d , d , or infinitely near, the maximum of decomposing effects must take place. This we observe is consonant to experiments.

Investigation of
the laws of de-
composition of
water by gale-
vanism.

If a particle of water consists of a particle of oxygen and of hydrogen in a state of chemical union, and if an agent be employed sufficiently subtle to penetrate the constituent particle of water, a separation will take place. Thus caloric will destroy the aggregation of the constituent particles of many substances, without producing any decomposition; in this case it only acts by penetrating the interstices formed by the constituent particles, without pervading the substance of the constituent particle itself. Thus the portions of water, although participating of the respective electrical intensities of the two wires, as yet undergo no decomposition; as yet it has not completely pervaded the particle of water. When a particle of water is influenced on one side by the positive wire, and on its other side by the negative, then the particle of water becomes a small Leyden phial; and if the energy of the apparatus should be adequate to overcome the resistance between the two sides of the particle of water, a separation is produced, and the water becomes resolved into oxygen and hydrogen.

This destruction of the water will take place at i , the intermediate distance between a , a ; because, when thus remotely situated, only a single particle of water becomes thus operated upon; when at b , b , a greater number of particles of water become thus influenced, and consequently the decomposition is more rapid.

If water placed in a capillary tube be employed, it is evident, however near the platinum wires are approximated, the areas of the bases of the respective cones will be not at all increased, and, consequently, the maximum of its power will be confined comparatively to a solitary particle of water. The instant a particle of water becomes thus separated into its elementary portions, it might be expected that these portions would be evolved at i , as being the point of concurrence between the positive and negative states; but in this form they are not in the gaseous state; they require caloric, &c.; and that they do not very readily acquire this other principle
necessary

necessary to constitute them gases, is evident from the oxygen, if an oxidable metal be employed at the positive end, entering into combination with the metal, instead of being disengaged in an aeriform state.

Water is a conductor of electricity; its capacity for this principle being in proportion to its conducting powers. When converted into vapour, its conducting powers are increased, and so also in the same proportion its capacity, as is evident from water, when undergoing this change, abstracting electricity from surrounding bodies.

When a particle of water is resolved into its elementary portions, a change takes place in their capacities for electricity, caloric, &c. at the very nascent instant. Oxygen we know, from other experiments, has its capacity for electricity increased, and must acquire this principle before it can assume a gaseous form. This acquisition will be effected with the greatest readiness in the direction towards the positive wire; while the hydrogen particle having its capacity diminished, will be similarly determined towards the negative wire.

For a future Journal I will trouble you with some experiments relative to this change of capacity. I am anxious first to peruse Mr. Gough's intended paper, from whose philosophical abilities much may be expected.

Having extended this paper to a greater length than I at first expected, I shall defer, till another opportunity, a reply to Mr. Cuthbertson's observations, and flatter myself I shall be enabled to clearly point out the error into which Mr. C. has fallen.

Cure of rheumatism and palsy by galvanism.

Although your Journal may not be deemed the proper medium for any medical communications, as I have lately witnessed the very powerful effects of galvanism in rheumatic and paralytic affections. Whatever may prove of benefit to mankind will, I am persuaded, be readily admitted into your valuable Work.

I am, Dear Sir,

Your's, with the greatest respect,

C. WILKINSON,

Surgeon, 19, Soho Square,
London.

Remark

VII.

*Remark upon an Assertion of LAVOISIER, which has been repeated by eminent Chemists. By P. PREVOST, Professor at Geneva, and Correspondent with the National Institute of France.**

“IT is a constant phenomenon in nature that when any body whatever is heated its dimensions become increased in every direction If after having heated a solid body to a certain point, and by that means separated its particles more and more, it be then suffered to cool, these particles will approach each other, according to the same gradation by which they were before separated; the body will pass through the same dimensions it before possessed; and if it be reduced to the same temperature as at the beginning of the experiment, it will resume its original volume. But as we are very far from being able to produce an absolute state of cold; but on the contrary, as there is no degree of refrigeration which we cannot suppose to be capable of being further augmented, it must follow that we have not yet succeeded in bringing the particles of any body as closely together as is possible, and consequently that *the particles of no body in nature can be in a state of contact*; a very singular conclusion, but to which nevertheless it is impossible to refuse our assent.”—*Elementary Treatise of Chemistry, by Lavoisier, Vol. I. at the commencement.*

The same assertion is repeated several pages further, where an example is taken of a vessel filled with small leaden shot. “The balls touch each other,” says our author, “whereas *the particles of bodies do not touch*, but are always kept at a small distance from each other by the effort of caloric.”

The authority which this celebrated chemist enjoys in the learned world, is probably the only cause why this conclusion has been adopted. It is at least certain that it is not legitimate, though it has been so often repeated; and more particularly in a late work, no less remarkable for the depth of reasoning and extent of knowledge which it displays. Ber-

Assertion of Lavoisier, that since bodies are contractible by cold, their particles are not in contact.

The same assertion repeated.

by Berthollet.

thollet, in his *Essai de Static Chimique*, Tom. I. p. 24, parag. 2, * has these words: " Cohesion is the effect of that affinity which the particles exercise on each other, and keeps them at a distance, determined by the equilibrium of this force with those which are opposed to it; for the property possessed by the most compact bodies of undergoing a diminution of volume by reducing their temperature, proves that there is no immediate contact between their parts."

Apology for
Berthollet,

After all, it is not surprising that an author, who has undertaken and successfully executed a work of such immensity with regard to chemistry, should refer to one of his most celebrated predecessors for the solidity of an argument which relates to the principles of natural philosophy; and this conduct is so much the more natural, as the argument never having been contested, though frequently quoted, a considerable presumption arises in its favour, to dispense with any careful examination in cases where it is not intended to be farther applied. Now it does not appear that any farther use is made of the proposition throughout the great work to which our attention is now directed. I therefore consider the passage here extracted, rather as the occasional mention of a singular paradox, than as a thesis which the author was desirous of establishing.

whose authority
may nevertheless
assist in estab-
lishing this
error.

Let this be as it may, it will notwithstanding follow, that the mere incidental mention of an assertion by an author to justly respected, will be considered as a proof of its truth; and that the numerous disciples of this great master will repeat it with confidence. It is therefore desirable that its want of foundation should be insisted on, and that it should be directly refuted in a Journal of extensive circulation; more particularly amongst those who are most likely to be misled by such an error.

The author does
not discuss the
proposition it-
self, but only in-
sists that expan-
sion and contrac-
tion prove
nothing.

Before we proceed, it will be proper to make an essential distinction. The assertion is as follows: *The particles of bodies do not touch each other*; or in other terms, *there is no immediate contact between the elementary parts of bodies*. Now, to speak with propriety, I do not at all intend to deny this assertion, neither do I propose to establish it. All that I intend to do is, to shew that it is not legitimately concluded

* See Lambert's Translation, Vol. I. p. 2.

from the principle whence it has been deduced; namely, the diminution of volume which accompanies depressions of temperature. Now I say that this diminution does not at all prove that the elements of bodies have no immediate contact, and consequently that this last assertion is so far gratuitous. And this is the whole of what I design to prove.

If it be true that there are numerous conceptions and ex-
amples which are evident and easy to be given, of bodies ^{We can easily conceive expansion without cessation of con-}
subject to dilation and condensation without their parts ceasing ^{tact.}
to be in immediate contact, it must necessarily be concluded, that dilation and condensation can afford no proof in this respect. For why should we refuse to admit some of these conceptions, or to apply some of these examples to the case of the elements?

1. Conceive the particles to be elongated and united by ^{1. Instance: the}
their extremities, like the legs of a pair of compasses; and ^{legs of compasses}
they may turn with regard to this point of union, as a centre, ^{may open and shut,}
and produce condensations and dilations of the whole apparent mass of the body successively.

2. A dry sponge, or fruit, or mucosity, being plunged in ^{2. Sponge, &c.}
water, will dilate without any cessation of the sensible con-
tact; and on the contrary, the same bodies, when wetted, ^{may be compressed or enlarged.}
if exposed to become dry, will undergo condensation.

3. The example of the dilatation of ice and some metals by ^{3. Expansion in}
crystallization, an example formally remarked, studied, and ^{crystals.}
well explained, particularly by the latter of the two learned chemists I have cited, must shew the falsity of the assertion which I refute.

Upon reading the work of Berthollet, and taking notes of such things as I was desirous of retaining, or concerning which I saw reason to doubt, I wrote what is here transcribed, *i. e.* the summary indication of the three conceptions, or examples, proper to elucidate this subject.

I had no intention of submitting my remark to its natural judges, when five or six weeks afterwards, being employed ^{Similar remarks by Lefage.}
in a very different occupation, I found in the papers of a learned * philosopher, which were entrusted to me at his death,

* George Louis Lefage, Correspondent of the Academy of Sciences of Paris, afterwards Correspondent of the National Institute, Member

death, a card which contained the same remark, but in a more abridged form, and by simple indication. As it offers a variety of examples and conceptions of the same nature as those which I have given, though nevertheless different, I shall here give them verbatim :

Note of G. L. Lefage.

“ Methods of shewing that this consequence does not follow :

- | | |
|--|---|
| Expansion and contraction,
1. Of the hands;
2. Combs, &c.

3. Cotton, &c.
4. Snow.
5. Oscillation. | “ 1. Four fingers of one hand introduced more or less between the four intervals of the fingers of the other hand.
“ 2. The same with a couple of brushes, or cards, or combs.
“ 3. Carded cotton, or hair, and soap lather.
“ 4. Snow.
“ 5. Agitation or oscillation, which renders the same particles alternately contiguous and separate more than a thousand times per minute.” |
|--|---|

Conclusion. The perusal of this note determined me to publish mine ; not only because I found myself authorized by this coincidence, but also to render a first homage to the memory of a philosopher no less modest than ingenious, and to begin in some respect to execute his will, by publishing at least one of his notes without making any change. The last conception offered in this last short note, will probably induce some philosophers to reflect again upon this subject, and may perhaps lead them towards the opinions of the writer.

Member of the Royal Society of London, and of several other learned Societies, died at Geneva, the 28th Brumaire, in the year XII. (Nov. 19, 1803). His principal writings have not yet been published. I shall soon give a short notice, as well as some details of his literary life, proper to facilitate the perusal of his works, and establish their originality.

P.

VIII.

Account of a Memoir on chamoying of Leather.

By M. SEGUIN.*

M. SEGUIN, who has already published several interesting works on the arts, relating to the preparation of skins, has lately read to the Institute a first memoir on chamoying, from which we shall give an extract.

The author states, that the art of chamoying consists in disposing the skins to receive the oil; in impregnating them with it by different operations, of which he probably reserves the details for a second memoir; in then causing them to undergo a species of fermentation; in exposing them to the air; and, lastly, in taking from them, by means of potash, the excess of oil which is useless to them.

He afterwards passes to the chemical examination of a chamoyed skin.

He found that this skin did not undergo any alteration by a long boiling in water; but that, if any acid whatever is added (M. Seguin made use of sulphuric acid), the skin disappeared entirely; that a certain quantity of a concrete oil swam on the surface of the liquid; that the liquor contained gelatine; and that, by its evaporation, it deposited crystals of sulphate of potash. He also ascertained, that on pouring gelatine into a solution of soap, an insoluble precipitate is obtained, which, treated with an acid, comports itself exactly like a chamoyed skin.

These results, and the considerations to which the exposition of the principal operations of chamoying have given rise, induced M. Seguin to conclude that the skins, in the fermentation which they undergo, yield a part of their oxygen to the oil; that the potash employed to cleanse them, forms a soap with the oxygenated oil; that one part of this soap, by combining with the disoxygenated skin, produces the insoluble substance which makes the skin chamoyed; and that the other part serves to constitute that grease which remains in the leather.

Mechanical operations of chamoying.

Chemical examination of chamoyed leather.

Gelatine poured into a solution of soap, forms a precipitate which resembles a chamoyed skin in its chemical properties.

Theory of the operation.

* From Bulletin des Sciences, Tom. III. p. 209.

This work is the more interesting, because hitherto no one has considered chamoying in a chemical view, and because the analysis to which M. Seguin has submitted his results, renders it susceptible of acquiring that perfection which the discoveries of this chemist have already given to the art of the tanner.

IX.

*Analysis and Decomposition of a Liquor employed to render Stuffs impermeable to Water. By M. VAUQUELIN.**

IT is known that, for some years, different persons have been successfully employed in rendering stuffs impermeable to water, an object of great importance in the clothing of soldiers and sailors.

The inventors of this process have hitherto kept the means they make use of secret: there was only reason to suppose that some fat oil was the basis of their receipts, but experiment has not yet developed it.

A bottle of this preparation, of which the efficacy was known, having accidentally come into my hands, gave me a desire to investigate its composition; but before stating the plan which I followed with this view, I shall describe its physical properties.

Physical characters of a liquor for rendering stuffs impermeable to water.

It is a white liquor, milky and opaque, of a bitter taste, and smelling like soap; on its surface there is a species of cream resembling that of milk, and it strongly reddens the tincture of turnsole. From these properties I thought it was simply a solution of soap, of which it still retained the smell and taste, which had been decomposed by an acid; but subsequent experiments soon convinced me that there was something more in it.

Chemical examination.

A turbid part is separated by repeated filtration.

First Experiment.—To find whether I could separate the white matter which rendered the liquid turbid, by filtration, I put a certain quantity of it on bibulous paper: it passed turbid and milky for a long time, but, by returning it several times on the same filter, I succeeded in obtaining it as clear

* From Bulletin des Sciences, Tom. III. p. 210.

as water; and I afterwards examined the liquor, and the matter which remained on the filter, separately.

Second Experiment.—If my conjecture was well founded, I should only find in this liquor the base of a soap united to an acid, of which there was a superabundance. My first care was to satisfy myself of the nature of this acid, and that which its taste had already indicated to me, was confirmed by muriate of barytes producing in it an abundant precipitate, insoluble in nitric acid: thus I was convinced that the liquor contained sulphuric acid; but, on the other hand, ammonia having produced in this liquor a white, flocculent, semitransparent precipitate, I saw that it contained something more than the salt resulting from the decomposition of soap.

Contains sulphuric acid.

Ammonia forms a white precipitate in it.

Third Experiment.—I then precipitated a certain quantity of this liquor; I washed the precipitate and dried it: as it had all the physical characters of alumine, I combined it with sulphuric acid; I added to it a little sulphate of potash, and, by a slow evaporation, obtained some very fine alum. We have therefore already found in this liquor alumine and sulphuric acid, doubtless combined in the state of alum.

Contains alumine, probably in the state of alum;

Fourth Experiment.—It was now requisite to know whether the liquor from which I had separated the alumine, did not still contain some other substance, and I therefore submitted it to some trials by the re-agents, among which the oxygenated muriatic acid and the infusion of nut-galls enabled me to discover a new body: the first rendered the liquor turbid, and soon afterwards produced white flakes in it; the second produced yellowish-white flakes, in much greater abundance than those arising from the effect of the muriatic acid; whence I suspected that, besides the matters mentioned above, this liquor contained an animal matter, and more particularly gelatine.

and also an animal matter,

Fifth Experiment.—To be better satisfied of the nature of this substance, I evaporated the liquor to dryness, by means of a gentle heat; I obtained a yellowish salt, of a bitter taste, which, by being re-dissolved in water, left a voluminous yellow matter, in the form of flakes, very glutinous, and acquiring, by drying, a sort of elasticity. This substance, placed on burning coals, swells and exhales white fumes, which have the odour of ammonia and fetid oil, such as are generally given by animal matters.

which is gelatine.

I no longer doubted that a certain quantity of animal gelatine had been put into this composition, with the intention of giving greater viscosity to the liquor, and enabling it to support, for a longer time and more effectively, the parts of the oil in suspension. It is probably by heat and by a commencement of decomposition, that the animal gelatine had become insoluble in water; but I perceived that the liquor containing the salt also held some of it in solution, for the muriatic acid and infusion of galls still formed precipitates in it, only less abundant than the first time.

Examination of the fat substance;

Sixth Experiment.—In this experiment I endeavoured to learn the nature of the fat substance retained in the filter, and of which I have spoken above: my intention was principally to discover whether it held some other substance in combination.

For this purpose I burned it with the filter in a platina crucible; it exhaled a vapour similar to that of tallow or the oils; it left some ashes, of which the filter had furnished a part: I found a small quantity of alumine in them, which could only proceed from the oil, for the bibulous paper did not contain an atom. I am also of opinion, that, in addition to the alumine, this oil contained a small quantity of animal matter, but I cannot positively assert it.

which retained some alumine in combination.

Thus, notwithstanding the excess of acid which existed in the liquor, the oil in precipitating carried with it, and retained in combination, some alumine, and probably some animal gelatine.

It is a combination of oil, alumine, and animal gelatine;

Hence the substance which, by uniting with stuffs, renders them *impermeable* to water, is not the oil alone, but a combination of this substance with the alumine, and probably with the animal gelatine, which renders this property more durable.

and yields a little soda and sulphate of potash.

Seventh Experiment.—The liquor which I had successively deprived of the oil, the alumine, and, in part, of the animal matter, by the different processes mentioned above, gave me, by a slow evaporation, crystals of a salt composed of soda and sulphate of potash.

Another analysis.

Eighth Experiment.—I made a better analysis of this liquor by another process, which I shall detail very briefly.

I precipitated the alumine and the oil by lime-water; I collected, washed, and calcined the precipitate: that which remained in the crucible was alumine and lime.

The liquor, from which these matters had been separated, evaporated to a certain degree, yielded sulphate of lime, a certain quantity of animal matter, become insoluble by the deficcation of the liquor, and, finally, sulphate of soda and potash, containing also animal gelatine, soluble in water.

The following is the manner in which I conceive this liquor was prepared, *with the exception of the proportions*: Soap and glue, or any other gelatine, was dissolved in water; a solution of alum was mixed with the solution of these substances, which, by its decomposition, formed in the mixture a flocculent precipitate, composed of oil, alumine, and animal matter; afterwards some dilute sulphuric acid was added, to redissolve the alumine in part, to render the precipitate lighter, and to prevent it from falling: but the alumine being once combined with the oil and animal matter, will not dissolve again entirely in the sulphuric acid, which is the reason why the oil continues to be very opaque, and neither rises nor precipitates: it will be understood, that too much sulphuric acid is not to be added. I do not know if this is precisely the manner in which it is made; I only know, that by following this process, I succeeded in composing a liquor exactly resembling it, and which possessed the same properties.

Manner of preparing the liquor.

X.

*New Experiments on Absorption by Charcoal, made by Means of a new Machine. By C. L. MOROZZO.**

1. IN the second memoir which I published on charcoal, in 1783,† I mentioned my intention of employing its property of absorbing the gases, to form a good eudiometer. I immediately set about the construction of an instrument with which I made some experiments; but the agitation of the times obliged me to abandon my pursuits: a philosopher, however, to whom I had shown this instrument, took the liberty of publishing it, notwithstanding its imperfections, and without naming the

First notice of the invention.

* From the Journal de Physique, Floreal, An. XII.

† Journal de Physique, p. 376.

author *. Let us pass over these unpleasant events, as well as the alterations and corrections which I made to bring the instrument to its present state: the description which follows, and the figure will be sufficient to show what it now is.

Description of the Instrument. (Plate XIV.)

Description of
the instrument.

II. A B is a tube of clear glass, three lines and a half in diameter and eighteen inches high: to this tube a stop-cock C is fixed, to shut the communication, by means of the key D, so that the tube may be filled with water or mercury, and the gas to be examined may be afterwards passed in. To this stop-cock C a large one E is soldered, an inch and a half in diameter, and almost three inches long. The handle F F serves to turn the key of the stop-cock E; this key has a hole, one inch and three lines in depth, and seven lines in diameter, (*Fig. 2.*) in which the charcoal is placed. This key is perfectly tight, and, when turned, communicates with the cock C, *Fig. 3*, not permitting the passage of any air whatever. The large stop-cock is enclosed in a wooden frame, and well secured by means of the nut H, which confines the lower end of the stop-cock E behind the board; the lower extremity of the tube dips into a vessel of mercury I I; the board M M keeps the instrument perpendicular; N N are pads which serve to raise the cup filled with mercury; O O is a scale mounted on a slip of wood, which is graduated to inches and lines. The endless screw P serves to raise or lower this scale, to bring it to the level of the mercury in the cup.

III. After having used the instrument for some time, it is necessary to unscrew the keys to clean them, for, by use, dust, charcoal, or ashes will get in, which deranges the instrument, and suffers air to pass.

IV. From several experiments, I found that the stop-cock D C, which is attached to the crystal tube should be of steel, for on examining the residual air, or filling the tube with mercury to examine any gas, the mercury will at length attack the copper.

* The author does not name the person here alluded to; and in a note he gives a description of this first imperfect instrument, referring to the figure so published. This is omitted, because of little utility and not intelligible without that figure. N.

V. I also think it right to propose a small alteration: it is <sup>Proposed im-
provement.</sup> that the extremity of the tube which is plunged into the bowl of mercury, should be a little more bent, that the residual gas may be transferred with greater ease: this slight variation, however, is not at all injurious to the instrument, because the heights are measured by that of the mercury, the level of which is indicated by the scale prepared for that purpose; its greatest inconvenience will be to make a larger and more cylindrical bowl necessary.

VI. This machine was made by J. B. Piana, a very skilful <sup>Accuracy of the
instrument.</sup> mechanic; he constructed it with such accuracy, that it preserved the most perfect vacuum for eight days, which is what I am unacquainted with in pneumatic machines.

Manner of using the Instrument.

VII. Suppose I am desirous of examining the absorption <sup>Method of
using it.</sup> effected in atmospheric air by charcoal. The tube A B is filled with the atmospheric air of the place in which the machine is: the stop-cock D is open, the upper one is closed: I place the cup of mercury under the tube, into which I insert a small glass syphon, to cause the air to take the level of the mercury. If I make use of a gas, I pump out the internal gas of the tube with a glass syringe, to bring the level within the tube to that of the exterior mercury in the bowl. Afterwards, by means of the screw P, I move the scale till its zero is also at the level of the mercury.

I make a piece of charcoal, weighing, half a dram or thirty-six grains, red-hot, and with tongs place it in the cavity G of the large stop-cock; I then turn the handle FF, and in a few minutes the mercury will be seen to ascend more or less in the tube, according as the air is more or less pure.

If it is wished to examine other gases, or atmospheric air brought from another place, then close the key D, and fill the tube A B with water; afterwards displace the water with the gas, in the common way by means of a transferrer; bring it to the mercury, and, by means of bibulous paper, absorb the water perfectly; then wipe the outside of the tube carefully with warm flannel, and leave the instrument at rest for two hours: or otherwise make use of a mercurial apparatus, which is preferable.

Inconveniences of the Instrument.

Inconveniences. VIII. Nothing is more certain than the property possessed by charcoal of absorbing a more or less considerable quantity of gas, and of atmospheric air; the experiments which I have published are a complete demonstration of it; but I then made use of white glass tubes, hermetically sealed at the upper extremity, and passed the charcoal under the mercury, by which its interstices were filled with that fluid. At present, on endeavouring to make comparative experiments with my new machine, which I was desirous should be a perfect eudiometer, I found that the cavity G, in which I placed the charcoal, and the small space between the two keys of the stop-cock, contained air; that by the heat of the charcoal this air was dilated, and partly expelled, and that consequently, on opening the key of the stop-cock, a small absorption was occasioned by the vacuum thus made, and the mercury then rose. To convince myself, and to discover at what quantity this absorption might be calculated, the apparatus being arranged as for other experiments, instead of charcoal I introduced a piece of red-hot pumice-stone (I gave the preference to this substance, because I was certain that it had not the property of absorbing the air, and also that it would not take a greater heat than charcoal): on turning the key an absorption of an inch took place, and from several experiments, I am convinced that this quantity is uniform within half a line.

Correction of the Instrument.

Correction of the instrument. IX. From the experiment made with the pumice-stone, it is obvious that if an inch is deducted from the total absorption, the real absorption produced by the charcoal will be obtained.

Besides, in comparative experiments this imperfection causes no error; because as they are all conducted in equal circumstances, the variations produced, either by the different gases, or by the different quality of the charcoal made use of, will be always proportional.

X. With this instrument I made several experiments which I shall now relate.

Order of the experiments. XI. I began by examining the action of charcoal when cold, and afterwards at a low heat; I then examined the absorption by

by charcoal exposed, for a greater or less length of time, to the light of the sun; this enabled me to ascertain the quantity of light and caloric which enter into the charcoal.

XII. The experiments on atmospheric air, which is more or less absorbed according to its purity, follow next.

XIII. After these are the experiments on different gases.

XIV. Oxygen gas gave me very singular results, diametrically opposite to those I had obtained in tubes hermetically sealed, and when I passed the charcoal through the mercury, which deranged me greatly in the consequences I deduced from it.

XV. After this article will follow some considerations on the quality of the charcoals made use of, which prove that their property of absorbing is different according to their different qualities.

XVI. I afterwards show that my instrument may be used in many cases, if instead of mercury, coloured water is employed, but with requisite modifications.

XVII. The charcoals which had been employed in the experiments all acquired a greater weight: I endeavoured to extract the air which they had absorbed.

XVIII. I also mention the trials which may be made by saturating the charcoal with different substances, having only made four experiments.

XIX. I then proceed to the conclusion, in which I endeavour to give the explanation of these experiments. The opinion which I had adopted in the second memoir, receives additional confirmation from it; but the results of the experiments I made on oxygen gas having changed the ideas I had formed, I am not willing to hazard an explanation at present. Philosophers will therefore be contented with having new facts and new experiments.

The experiments which I detail were repeated several times, and their accuracy may be depended on, as I can assure my readers, that I have always considered truth as the brightest ornament of a philosopher.

Experiments made with my new Instrument.

1st. The charcoal which I generally employed was that of beech-wood; the weight of the pieces half a dram, that is to say, thirty-six grains. Experiments with the new instrument.

2d. The tube of my glass was eighteen inches long, from the bottom of the key to the surface of the mercury, the level of which is indicated by the moveable scale. The diameter of the tube is three lines and a half: it contains one ounce and seven drams — five grains of water, or a volume of atmospheric air of four grains and a half at a temperature of $+10^{\circ}$.

3d. Being desirous of throwing a light upon the property possessed by charcoal of absorbing a part of the atmospheric air, as well as of several gases, by means of my new instrument, I made a great number of experiments, which I shall now describe.

4th. Charcoal, when cold, has the property of absorbing some small portions of atmospheric air, and this absorption, although very slow, is not complete in less than twenty-four hours.

On charcoal
heated without
the contact of
fire,

5th. Wishing to try whether charcoal to which heat had been communicated without direct exposure to fire, would show any absorbent power, I placed a piece in a small matrafs, which I immersed in boiling water for an hour; having afterwards put the charcoal into the machine, I obtained an absorption of about three inches.

6th. I boiled oil, in which I left the matrafs for an hour; an experiment with this charcoal gave an absorption of three inches two lines.

7th. A small matrafs containing a piece of charcoal was immersed for an hour in boiling alkaline lixivum: it gave an absorption of three inches three lines nearly.

These three experiments show that heat communicated to charcoal, even without the contact of fire, gives it the property of absorbing a larger portion of air.

and exposed to
the solar light.

8th. The preceding experiments induced me to examine whether the light of the sun would communicate the same property to charcoal, I therefore exposed different pieces to this light, in a white porcelain bowl, and the absorptions were proportionate to the times which the pieces were exposed to the light of the sun*.

* At the instant of putting a piece of charcoal into the machine, I placed another which had been exposed with the rest to the sun for three hours, on the bulb of a thermometer; it only raised one degree and a half of Reaumur's scale: the heat therefore was not very considerable.

After

After two hours the charcoal absorbed one inch two lines.

After three hours, one inch eight lines.

After five hours, two inches.

After seven hours, three inches nearly.

It is difficult to obtain very precise results, because the heat of the sun is greatly varied, according to the wind; but in this case it is light as light which unites with the charcoal, for in very cold days when the thermometer in the shade was at 6° , I still obtained an absorption*.

9th. Being desirous of ascertaining whether this absorbent property belonged to charcoal exclusively, I tried several other bodies, which there was reason to believe contained a great deal of the matter of fire.

10th. I took a small cylinder of loaf sugar, cold; it did not produce the smallest absorption. Experiments on loaf sugar,

A similar piece, which I had heated in a matras with boiling water, gave an absorption of between two and three lines.

A piece of sugar which had been exposed to the sun for two hours also gave an absorption of between two and three lines.

11th. I then proceeded to the examination of sulphur and on sealing wax.

A cylinder of sulphur of the same weight gave no absorption when cold.

A similar cylinder, which had been exposed four hours to the light of the sun, gave an absorption of about three lines.

A piece of cold sealing wax did not give the least absorption, and on sealing-wax. and a similar piece which had been two hours in the sun, gave an absorption of about two lines.

12th. All these small absorptions which were observed as well in the sugar as in the sulphur and sealing wax, were only owing to the slight heat of these bodies, which produces a dilatation in the air of the cavity, and a displacing of that portion of the air which is filled by the solid body; for a piece of pumice stone, of cork, or of any other body, produces the

* It would be interesting to examine the absorptions which might be produced by different pieces of charcoal which had been in the light of the sun transmitted through coloured glasses, or by making the different coloured rays fall on the charcoal by means of a prism, and to observe whether the piece exposed to the red ray would absorb more or less than that which had received the violet ray, and so of the others.

same

Charcoal alone
possessed of this
absorbent pro-
perty.

same effect. It is therefore in charcoal alone that this absorbent property resides, and it is developed to the highest degree by heat and light.

Experiments on Atmospheric Air.

Experiment
atmospheric

13th. I then proceeded to examine the effects of incandescent charcoal on atmospheric air, in my instrument, and I repeated the experiments several times: the air of my garden was constantly less absorbed than that of my bed-room, an hour after the windows had been opened, and this was less absorbed than that of my bed-room in the morning before opening the windows: the absorptions were,

The air of the garden, seven inches six lines.

The air of the room after the window had been open, eight inches one line.

The air of the room, in which it had not been renovated, eight inches six lines.

Former experi-
ments.

14th. In the experiments which I made in the year 1783, with tubes twelve inches in height, and one inch in diameter, I passed the charcoal through the mercury; the absorption of the atmospheric air was then always about three inches and six lines, that is to say, a little more than one fourth: in this case it is uniformly greater and about one third. Probably the mercury filled many of the interstices of the charcoal when it was passed through it, which might prevent this greater absorption.

Air of a privy.

15th. I afterwards examined the air of a privy, which was taken into the syringe seven toises above the sewer.

The absorption was eight inches, that is to say, nearly equal to that of my room. This did not surprize me, because in those compositions which contain azote, this gas is not much absorbed, as we shall presently see: besides, Mr. White with an eudiometer of nitrous gas also found that the air of a privy, gave him an absorption equal to that of common air*.

Experiments on the Gases.

Experiments on
the gases.

16th. After having examined the atmospheric air, I proceeded to the examination of carbonic acid gas: I filled the

* See Journal de Physique, Tom XVIII. p. 145, and the reflections of Guyton Morveau in his excellent treatise on the means of disinfecting the air.

tube with this gas, which I had extracted from powdered marble by vitriolic acid. I placed the incandescent charcoal in the machine which effected an absorption of sixteen inches six lines, that is to say, of eleven twelfths of the capacity. This experiment corresponds exactly with that which I made with the tube of twelve inches in height, and one inch in diameter, and in which the charcoal was passed through the mercury: the absorption in that case was eleven inches.

17th. I afterwards examined the effect produced on the same gas by a piece of charcoal which had remained five hours in the solar light. It caused an absorption of ten inches and three lines, which surprized me greatly.

18th. But to ascertain whether charcoal has any peculiar affinity with carbonic acid gas, I tried this gas by placing a piece of cold charcoal in the machine, and the absorption was nine inches six lines.

19th. To discover the effect of carbonic acid gas when mixed with atmospheric air. I filled the tube, which is eighteen inches high, with nine inches of carbonic acid gas, and nine inches of the air of my room with the windows open: the charcoal effected an absorption of twelve inches and three lines.

On analysing this experiment, it agrees perfectly with those related above, for

The absorption of the air of my room was 8 inches 1 line.

———— of the carbonic acid gas 16 6

Total 24 7

The half of this is twelve inches three lines and a half, within half a line of the absorption obtained.

20th. I afterwards tried the action of charcoal in hydrogen gas, obtained from iron by sulphuric acid, and the absorption was three inches and one line: it was accomplished in five or six minutes, and stopped at that height: the effect observed in my machine did not surprize me, for I had noticed in my first memoir, that of all the aeriform fluids, hydrogen gas was absorbed the least. There is a perfect correspondence between this experiment, and that which I made in the year 1783, in crystal tubes of one inch in diameter and twelve in height: the absorption was then two inches and one line, that is to say, one sixth of the height; in my new machine, the tube of which is eighteen inches long, the absorption was three inches and one line, which is also the sixth of the capacity.

21st. On

21st. On trying the same hydrogen gas with a piece of charcoal which had been kept about four hours in the bright solar light, I had only an absorption of six lines.

Azote gas.

22d. I next proceeded to the examination of azote gas. I began by examining that obtained from atmospheric air, in which a candle was extinguished.

The absorption was six inches.

23d. I examined that obtained by the combustion of sulphur, and had an absorption of seven inches and three lines: it is to be observed that this gas always contains sulphureous acid, and for that reason more of it was absorbed.

A second experiment with the charcoal gave me seven inches and two lines.

I afterwards tried the azote gas obtained from atmospheric air in which I had extinguished a large piece of charcoal under a bell: the absorption was seven inches six lines; but this gas always contains a little carbonic acid, and consequently there is a larger absorption.

24th. To be sure of having the purest azote gas without mixture, I took the gas obtained by decomposing nitrous acid by means of hydrogen gas, which last gas had been obtained from the decomposition of water by iron.

The absorption was only six inches and one line.

We have not yet very correct ideas with respect to azote: it appeared to me, however, in several of the experiments which I made with atmospheric air, that when it is united with aqueous vapour, more of it is absorbed than when in a dry state. Besides it is evident that the azote gas is not much absorbed, which corresponds with my first experiments.

(The Conclusion in our next.)

XI.

On the Commerce of Hens Eggs, and on their Preservation. By
M. PARMENTIER *.

THERE are very considerable differences in the eggs of hens in respect to size. Some are as large as the eggs of ducks, while others resemble those of pigeons. M. Parmentier, from

* From Bulletin des Sciences, Tom. III. p. 214.

observations

The bulk of the egg does not depend so much on the nourishment as on the breed of the hen,

observations on a great number of hens of different breeds, which he reared in the same place, has found that the bulk of the eggs depends much more on the breed of the hen than on the quantity of nourishment. Those breeds which give the largest eggs are not however to be preferred on that account, because with them as much may be lost in the quantity of eggs as is gained by their size. Of all the breeds known in France the author gives the preference, with respect to the produce of eggs, to that which is called the common hen, and which is only common because its merit is known. Those which have black legs are in greater esteem than those with yellow ones.

From comparative experiments continued for a year, M. Parmentier found that though the eggs of this breed were not so large as some others, yet every thing besides being equal they produced at least one half more.

Next to this breed of hens comes the crested hen and the large Flemish hen. The one is more delicate eating, because laying less than the common hen, it grows fatter; the other, without being more productive, is preferable for raising chickens. The *silky* hen (*poule de soie*) so beautiful in its form and in the fineness of its plumage, so careful in laying, so assiduous in setting, so tender of its chicks, might be recommended, but unfortunately two of its eggs are not worth one common egg. This circumstance places it among those which must be left for the curious.

After the choice of the breeds, care must be taken that the hens are neither fed too abundantly nor too sparingly; that they do not wet their feet, that they are sufficiently close in the roost to heat and electrify each other; and, that they meet with a little warm dung in the day time.

When the only object in keeping hens is to procure eggs, and thus turn to profit the grain which remains among the chaff and in the dung-hills, it is wholly useless to keep cocks at the same time, because experience has shown that hens deprived of the male do not lay less than those which have it. The saving by this is not the only nor the greatest advantage. The unfecundated eggs keep much better than those which are se- Unfecundated
cundated. It has been found by experiment that they can sup- eggs least liable
port a heat of 32° (87° F.) for thirty or forty days without experi- to spoil.
encing any change. It is therefore evident that the evaporation of the liquors is not, as Reaumur thought, the immediate cause

cause of the putrid change in eggs, and that to preserve them, it is not sufficient to cover them with grease or oil, as this learned man recommends, since in the experiment mentioned above the unfecundated eggs did not spoil, though they lost considerably by evaporation. The fecundation, by the principle of life which it communicates to the germ, exposes the eggs to many accidents which do not take place with those in which the male has had no share.

Causes of the
spoiling of eggs.

M. Parmentier particularizes some of these accidents. Some of them arise from the commencement of the development of the germ. Sometimes it is enough if several hens lay their eggs in the same nest; for the egg which is first laid in it, partaking in succession, and for some hours, of the heat of the hens which follow each other, undergoes a species of incubation which excites the vitality of the germ, and this egg becomes changed, though it is but recently laid. It is thus that eggs of the same age appear frequently to differ in their freshness. At other times the change in the egg may arise from the fecundating germ being killed, either by thunder, or in the conveyance, by the jolting of the carriage or the rolling of the vessel, or by the lapse of time. When the germ is once dead it corrupts, and communicates the corruption to that

Utility of im-
mersing the eggs
for a short time
in boiling water.

which surrounds it. This theory appears to explain one method I made use of successfully for preserving eggs even when fecundated: it consists in plunging them for a couple of seconds into boiling water. It is known that by this means they become susceptible of being preserved for several months, if they are afterwards kept in a cool place, or in salt. M. Parmentier suspects that the utility of this process depends on the destruction of the vitality of the germ by the boiling water.

Eggs laid at sea
keep longer than
others.

Mariners assert that eggs laid at sea keep better than others. May not this arise from the hens on board a ship having no communication with cocks? For the same reason the diminished vigour of the cocks in our poultry yards, in autumn, may be one cause of the eggs laid in that season being more capable of being kept than those of the first laying, to which may be added, that the hens are at that time fed more with grain and less with herbage.

Requisites for
keeping eggs
fresh.

From these observations, M. Parmentier thinks that the first condition towards obtaining eggs capable of being preserved
and

and transported without spoiling, is not to keep a cock with the hens in the poultry yard. It is an error to suppose that eggs not fecundated have a worse taste than those which are so. The author ascertained that the most delicate palate was incapable of discovering any difference. Afterwards it is only requisite to shelter the eggs from humidity, light, heat, and frost. The method which succeeded best with the author was to procure baskets made of straw, in which he placed the eggs with layers of chaff between them. The straw and the chaff are dry materials, smooth, very bad conductors of heat, and consequently, very proper to preserve the character of freshness in the eggs: these baskets were afterwards suspended in a dry, dark and airy situation.

XII.

Method of giving the Colour, Grain, and Hardness of Steel to Copper. By B. G. SAGE*.

MARGRAFF and Pelletier have published their investigations relative to the union of phosphorus with different metallic substances; the French chemist has brought this process to perfection: it was by repeating and varying his experiments that I discovered that the readiest and most certain method of phosphorating copper is to take the copper in the metallic form, to melt it with two parts of animal glass, and a twelfth part of powdered charcoal. But it is essential that the copper should offer a large surface; an advantage which is obtained by using chips of this metal, which are to be placed in alternate layers with the animal glass mixed with the powdered charcoal. I expose the crucible to a fire brisk enough to melt the animal glass; it forms phosphorus; the greatest part of which burns, while another part combines with the copper, in which it is so consolidated that it does not quit it, although it is kept in fusion for twenty minutes under the animal glass which has not been decomposed.

When the crucible has cooled and is broken, the phosphorated copper is found under the glass, which has passed to the

* From Journal de Physique, Messidor, An. XII.

state of a red enamel, in the form of a grey and brilliant button: on weighing it, it is found to have acquired one twelfth by this operation.

Its action on polished iron.

If the melted phosphorated copper falls on a plate of polished iron, it spreads itself in the form of plates of various configurations which are iridescent like the neck of a pigeon.

Is more fusible than copper.

Phosphorated copper is much more fusible than red copper: it may be often melted under charcoal powder without losing its properties.

Does not quit the phosphorus without difficulty.

The same phosphorated copper, exposed for a long time under a muffle, does not separate from the phosphorus without great difficulty.

Resembles steel in hardness, grain and colour, and does not tarnish by exposure to the air.

The copper thus combined with the phosphorus acquires the hardness of steel, of which it has the grain and the colour; like it, it is susceptible of the most beautiful polish; it is turned easily in the lathe; it does not change in the air: I have for fifteen years, kept buttons of polished phosphorated copper in my laboratory, which have not experienced the least change. The copper does not develop any smell on being rubbed: if it was ductile it would be of the greatest utility, since fat bodies do not seem to make any impression upon it.

A small quantity of charcoal to be used in its preparation.

In the phosphoration of copper there is only a part of the animal glass decomposed: because a sufficient quantity of charcoal is not employed to convert all the acid into phosphorus; but it is necessary that this should be the case, that the phosphorated copper may separate and collect with facility.

Red enamel.

The deep red enamel which is formed in this experiment may be advantageously employed for porcelain or the enamels, since it does not change colour in the fire.

Copper and phosphorus can only be combined in the dry way.

Copper cannot combine with phosphorus except in the dry way. If a cylinder of phosphorus is put into a solution of nitrate of copper diluted with four or five thousand parts of water, at the end of eight days the copper will be found in a metallic form, crystallized and ductile, forming a case for the cylinder of phosphorus.

XIII.

*Observations on Mr. GOUGH's two Letters on Mixed Gases.**

To Mr. NICHOLSON.

SIR,

MY reply to Mr. Gough's strictures being chargeable, it seems, with acrimony and ridicule, and with having but few arguments, and those in appearance negligently conducted. (though the last charge requires a whole letter to make it good); I propose, in what follows, to avoid the two former of these as much as may be, and to be as careful as possible in conducting my arguments; so, that if they appear to Mr. G. destitute of that logical precision which characterises his, he may ascribe it to my inability, and not to any want of inclination.

The accuracy of Mr. G.'s demonstration in the former letter, depends upon that of three physical data: 1st, The specific gravity of azotic gas; 2^d, The specific gravity of oxygenous gas; and, 3^d, The quantity of oxygen in a given volume of atmospheric air. If any one of these be wrong, it may prove fatal to his demonstration: now it unfortunately happens that all three are wrong, and that, when corrected, they prove the very reverse of his proposition; namely, that atmospheric air is a mechanical mixture of oxygenous and azotic gases.

Mr. G.'s data are,

Sp. gravity of azotic gas, .985

----- oxygenous gas, 1.103

Quant. of oxygen in atmo-

sphere, per cent. in bulk, 22-25-28, uncertain, which.

The true, or at least the most approximate numbers, are,

" Sp. gravity of azotic gas, .966

----- oxygenous gas, 1.127

Quant. of oxig. in atmosphere,

per cent. in bulk, - - 21

Then, per Mr. G.'s theorem, $21 \times 1.127 = 23.667$

and $79 \times .966 = 76.314$

99.981

* See Pages 107, 160, of the present Vol.

Observations on
Mr. Gough's
letter on mixed
gases.

The sum is so near 100, that Mr. G. will not venture to presume any thing on the difference, further than that the data are still in a small degree incorrect, which I believe no body will dispute with him.

Mr. G. by this time is ready to query, How do you know your data to be more correct than mine? I will now inform him.

Dr. Priestley was perhaps the first to investigate the specific gravities of the two gases in question. His method was very exceptionable; but such is generally the case in the infancy of any science. (*See Vol. II. Page 452, abridg. Ed. of his Nat. Philos.*) He found azotic gas as much lighter than common air as oxygenous was heavier. Mr. Kirwan soon after gave a much nearer approximation; namely, the one which Mr. G. has adopted. Lavoisier also found the specific gravity of several gases. (*See Elements of Chemistry, Append. Table 7*). His results nearly agree with Kirwan's in regard to oxygen, but differ considerably in regard to azote. Lastly, Mr. Davy, when investigating the compounds of azote and oxygen, found it expedient to ascertain with precision the specific gravity of the two gases. Having every means of his predecessors, and their results before him, he ought at least to have decided in regard to the difference between them. Accordingly he finds his results to agree with Lavoisier's in respect to azote; but he finds the specific gravity of oxygen somewhat greater than either of them. (*See Recherches, pag. 555*).

The following table exhibits the results of all these together, reduced to the standard of atmospheric air.

Specific Gravities of

	Azotic Gas.	Atmosf. Air.	Oxygenous Gas.
According to Priestley,	.989	— 1.000	— 1.011
Kirwan,	.985	— 1.000	— 1.103
Lavoisier,	.966	— 1.000	— 1.102
Davy,	.966	— 1.000	— 1.127

As for Mr. G.'s third datum, I think it scarcely pardonable in the present day, for any one to pretend to discuss a question concerning the constitution of the atmosphere, under the uncertainty whether it contains 22 or 28 *per cent.* of oxygen. He ought to be acquainted with the history of eudiometry,
and

and to repeat experiments considered as decisive, especially as they are of a simple nature. All the chemists of Europe seem to be agreed, that either 21 or 22 *per cent.* in bulk, is the proper number. My own experience gives 21 for the nearest integer, which agrees with Mr. Davy's in your Journal, (quarto Series), Vol. V. page 175 : if Mr. G. entertains any doubt on the subject, I would refer him to that paper, and he will receive ample satisfaction.

Observations on
Mr. Gough's
letter on mixed
gases.

After what has been said, I think Mr. G. must be satisfied that he has virtually demonstrated the atmosphere to be a mechanical mixture of the two gases: If, however, he still alledge that I only oppose one authority to another, and that his is as good as mine; then I would recommend him to satisfy himself as follows: mix 21 parts (or a quantity of gas containing 21) of pure oxygen, and 79 of pure azote, together; after this proceed to the analysis of the gases, and examine the differences between the results, and those done from the analysis of 100 parts of atmospheric air.

I cannot conclude this article without observing, that Mr. G. must have been totally unacquainted with the opinions of chemical philosophers on the subject, or he would not have expanded into the compass of five or six pages a simple argument, which has often been adverted to by others, and is now wholly abandoned as untenable. Mr. Davy, in his *Researches*, published in 1800, advocating the notion of atmospherical air being a chemical compound, produces four evidences, one of which is stated as follows: See Page 326.

“2dly, The difference between the sp. gr. of atmospheric air, and a mixture of 27 parts oxygen and 73 nitrogen, as found by calculation; a difference apparently owing to expansion in consequence of combination.” In a note he adds, “The two first evidences have been often noticed.” This gentleman however soon after, finding that the atmosphere contained only 21 *per cent.* of oxygen, must have seen that this evidence was not to be admitted. Since that time it has not been urged by any one to my knowledge.

I come next to Mr. G.'s reply at page 160. This at the commencement purports to be a defence of the charge, that *my arguments are but few, and negligently conducted*; at the conclusion it is asserted to be an *answer to all my objections, and something more*. This language may be that of logical precision

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precision with Mr. G.; but it would have been more intelligible to me if he had stated the objects of his reply thus:

1st, A refutation of Mr. D.'s arguments in favour of his system.

2^d, An answer to his objections brought against mine.

3^d, New objections to his system.

The first thing worthy of notice is the objection to my argument for the mutual penetrability of gases. I have assumed *one* postulate and taken *two*, and the latter of them is erroneous, namely, that *all gases are porous*. It is true, I have taken two postulates into the argument, without expressly requiring both; the former being peculiar to my theory, was necessarily demanded in a formal way; the latter being the result of all experience, and never in any one instance having been found to fail, I thought it might *tacitly* be assumed. However, the judgment of philosophers must be suspended on this head, as Mr. G. it seems, is about to prove that *no gas is porous*, and that *a cubic foot of one gas cannot be put into a vessel that is previously occupied by another gas*. Mr. G. surely cannot be serious in this objection; but merely uses it to gain time, and means to turn it off with a laugh, that he has at least produced one *solid* argument against my *airy* hypothesis.

Mr. G. finds it extremely convenient for his purpose, that I should grant him the following postulate: "If a particle of vapour can pass freely through the air, a second can also succeed it at any given distance." I certainly cannot concede so indefinite a demand; but it will perhaps be of equal use to him to have the following: If *d* be the distance of two particles of vapour of the temperature of 212° , and pressure 30 inches; then, at the temperature of 60° or upwards, if one particle of vapour can pass freely through the air, a second may succeed it at any distance greater than $4d$.

As for the important argument which I consider equivalent to a demonstration of the nature of vapour, and of its relation to gases, and entirely inimical to the notion of chemical affinity, Mr. G. has not ventured to revive it: Probably he has something in reserve on this head. I mean the argument derived from the fact, that a vacuum, or any kind of air of any density whatever, takes up just the same quantity of any vapour.

The

The proposition respecting the mechanical action of air on the surface of water, will be answered by Mr. G. when he has proved that *air has no pores, or no capacity for the reception of water.* For, nothing can be more clear than that, whatever may be the pressure of the atmosphere, and however few the points of its action, water cannot be forced into the pores of air, if it have none.

Mr. G. proceeds to state two new facts, which are said to be inexplicable on my principles: I must undertake the arduous task. The facts are, 1st, Air containing aqueous vapour is specifically lighter than air without it, *ceteris paribus*; and, 2^d, A bottle internally moist, containing air, being heated, more air is expelled than if the bottle had been dry. Both granted.—Now for the explication. The specific gravity of aqueous vapour has been found by De Saussure, Watt, and others, to be about $\frac{3}{4}$ or $\frac{1}{2}$ of that of atmospheric air in like circumstances; by some experiments of my own I am induced to think it is nearly .7, that of air being 1. Let the temperature be 64°, and the air be filled with vapour as much as possible in the temperature, in which case $\frac{1}{3}$ of the elastic force will be due to the vapour. (See *Manch. Mem. Vol. V. P. 2, page 559*); then, by the theorem so elaborately exemplified in Mr. G.'s former letters, we have
$$\frac{49 \times 1 + 1 \times 7}{50} =$$

$\frac{49.7}{50} = .994$, for the specific gravity of common air filled with vapour at 64°, when that of dry air of the same temperature would be 1. Thus it appears that my hypothesis not only explains the fact of diminished specific gravity, but accounts for the quantity of diminution. Can Mr. G.'s theory of chemical solution do this?

In the second case we find heat generating vapour, which increasing in quantity and force with the temperature, diffuses itself through the air in the bottle; the hand being occasionally removed from the mouth, suffered the extra-portion of air and vapour to start out; just as if there had been a generation of a like portion of oxygen, or any other permanent gas, instead of the vapour; in which case a portion of both does certainly escape. Mr. G. finds the vapour generated this way between 59 and 126°, to be rather less than $\frac{1}{2}$ of at-

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atmospheric force; it ought to be just $\frac{1}{2}$ by the table above referred to. But it is obvious, that by removing the hand occasionally, the *full* quantity of vapour for any temperature could never be obtained in this way, no more than pure oxygen could be procured in the bottle by a similar process. The grand question with Mr. G. however, is, How does vapour of half an inch force, expand the pores of air subject to 30 inches of pressure? And his answer seems to be, it is impossible, according to the axioms of dynamics. This question is what I shall now consider.

Having myself studied the principles of dynamics, as well as those of many other mathematical and physical sciences, under the tuition of Mr. Gough, I feel under strong obligation to him; but these, he will readily grant, do not bind me to subscribe to his opinions, when I cannot perceive them to be well founded. He charges me, in the present instance, with a mistake in regard to dynamics; but he has not pointed out any particular axiom which I have offended: The mistake, I think, is with him, and shall endeavour, in what follows, to point it out.

It is a principle in dynamics, that whenever a system of bodies act upon each other, and are in a state of equilibrium, the *least* force impressed upon any one disturbs the equilibrium. Thus, the ocean and the air, though bound to the earth by its superior attraction, are nevertheless disturbed by the feeble influence of the moon. Air in a bottle is a system of particles at equal distances repelling each other, but in equilibrium by the gravity of the incumbent atmosphere; consequently the *least* force impressed upon them must disturb that equilibrium. Now, *ex hypothesi*, air does not repel vapour at a distance, but only in contact; therefore vapour can be formed in such a system: when once formed, it meets with no elastic resistance or repulsion but from particles of its own kind; therefore it is constantly tending outwards, where the particles of its own kind are least dense: in its way it infringes upon particles of air, and exerts such force as it is capable of upon them: a number of particles of air are thus gently propelled in the direction of the stream, and the rest of the system are obliged to accommodate themselves in order to preserve the equilibrium: thus the distances of the particles of air are gradually

dually increased, and that in proportion to the force which the vapour exerts. Therefore vapour of the least possible force^{*} can, in such circumstances, extend the pores of air. Q. E. D.

I remain your's,

J. DALTON.

Manchester, Nov. 15, 1804.

XIV.

Some Account of a Condenser of Forces, or a Method of obtaining the greatest possible Effect from a first Mover, of which the Energy is subject to Increase, or Diminution within certain Limits; and in general to vary at Pleasure the Resistance to which the Effort of the first Mover forms an Equilibrium in any Machine whatever, without changing any Part of the Construction. By R. PRONY.*

THE problem of mechanics, of which the solution is here ^{Problems in} given, is one of the small number of those which, leading to ^{mechanics,} results independent of the particular mechanism of the machine to which they are applied, present, in their solution, a generality which may be compared with that of the rational mechanics, or analysis.

It may be enunciated in the following terms :

“ Any machine being constructed, to find, without making ^{enunciated.} any change in the construction, a means of transmitting to it the action of the first mover, by fulfilling the following conditions; viz.

“ 1. That it may be possible at pleasure, and with great ^{Conditions} speed and facility, to vary the resistance (against which the effort of the first mover must continually make an equilibrium) in limits of any required extent.

“ 2. That the resistance being once regulated, shall be rigorously constant until the moment when it is thought proper to increase or diminish the same.

“ 3. That in the most sudden variations of which the effort of the first mover may be capable, the variation in velocity of the machine shall never undergo a solution of continuity.”

* From the Bulletin of the Philomathic Society at Paris, No. 83.

Solution exhibited in an engine.

Problem in dynamics: To oppose a constant weight to a variable first mover, and transmit the force, &c.

I shall here apply the solution which I have discovered of this problem, to the dynamic effect of wind; it will be easy to make the same general when the other first movers are used.

The section and plan of the machine are exhibited in Plates XV. and XVI. OO represents the vertical arbor to which windmill sails are adapted; $ecce$ is an assemblage of carpentry, of which one of the radii Oe , bears a curved piece bd , of iron or steel: Vertical axes of rotation aaa , being placed round the axis OO , also divide the circumference in which they are found into equal parts.

Each of these axes carries a curve af , of iron, steel, or copper; so situated, that when the wind acts upon the sails, the curve bd presses against one of the curves af , and causes the vertical axis to which this last curve is fixed, to make a portion of a revolution.

The curves bd and af must be so disposed, that when bd ceases to press on one of the curves af , it shall at the same instant begin to act upon the following curve: the number of axes which are provided with these curves, must be determined by the particular circumstances of each case; and it is also practicable to substitute, instead of bd , a portion of a toothed wheel having its centre in the axis OO , and to place portions of pinions instead of the curves af , but the dispositions represented in the figure are preferable.

Each of the axes aaa (which are all fitted up alike, though, for the sake of clearness, only one of them has its apparatus represented in the drawing), each of these carries a drum or pulley $ttrr$, on which is wound a cord that passes over a pulley p , and serves to support a weight Q by means of the lever FG , upon which this weight may be slid and fastened at different distances from the point of motion G .

The same axes aa pass through the pinions qq , to which they are not fixed; but these pinions carry clicks or ratchets, which bear against the teeth rr ; so that, when the weight Q tends to rise, the ratchet gives way, and no other effect is produced on the pinion qq , either by the motion of the axis or of the drum $ttrr$, excepting that which causes the ascent of the weight qq . But the instant that the curve or tooth bd ceases to bear against one of the curves af , after having caused the corresponding weight Q to rise, that weight Q tends to redescend, and then the toothed wheel rr acts against the

the ratchett, so that Q cannot descend without turning the pinion $q q$ along with the drum $t t r r$.

The pinion $q q$ takes in the wheel $a b$, from the motion of which the useful effect of the machine immediately results; so that the effect of the descent of one of the weights Q , is to solicit the wheel $A B$ to motion, or to continue the motion in concurrence with all the other weights Q , which descend at the same time. This wheel $A B$ carries beneath it oblique or bevelled teeth $G D$, which take in a like wheel $C E$, and cause the buckets at S to rise.

The alternation in the motion of these buckets may be effected by the mechanism I have described in the first volume of the Memoirs of the Institute.

From the preceding description it is seen that the machine, being supposed to start from a state of repose, the wind will at first raise a number of weights Q , sufficient to put the machine into motion, and will continue to raise new weights while those before raised are fallen; so that the motion once impressed will be continued.

Among the numerous advantages of this new mechanism we may remark the following:

1. No violent shock can take place in any part of the mechanism.

2. The useful effect being proportioned to the number of weights Q , which descend at the same time, this effect will increase in proportion as the wind becomes stronger, and causes the sails to turn with more velocity.

3. The weights Q being moveable along the levers $F G$, it will always be very easy to place them in such a manner as to obtain that ratio of the effort of the first mover to the resistance, as shall produce the maximum of effect.

4. From this property it results, that advantage may be taken of the weakest breezes of wind, and to obtain a certain product in circumstances under which all other windmills are in a state of absolute inactivity: this advantage is of great importance, particularly with regard to agriculture: the windmills employed for watering lands are sometimes inactive for several days, and this inconvenience is more particularly felt in times of drought. A machine capable of moving with the slightest breeze, must therefore offer the most valuable advantages.

Problem in dynamics: To oppose a constant weight to a variable first mover, and transmit the force, &c.

I shall give a more ample account of this apparatus in a memoir which I shall present to the Institute, as soon as the machine which I am now erecting in the country shall be completed. *

XV.

Abstract of a Memoir on the Possibility of obtaining Prussiate of Potash free from Iron; the Unalterability of the Prussic Acid at high Temperatures; and the true Nature of the Combinations of this Acid with different Bases.† By BUCHOLZ.

Utility and advantages of prussic compounds.

THE combination of prussic acid with saleable bases, forms a class of salts of the greatest utility to the analytical chemist; for by means of them he is not only enabled to ascertain whether a metallic substance be present in any solution which forms the subject of his research, but also what metal is present, as well as its respective quantity. But in order to be accurate in this respect, the re-agent employed must itself be free from metallic admixture; or the quantity and nature of the metal it contains must at least be known. To accomplish this, chemists have hitherto laboured in vain. To remedy these defects, Mr. Bucholz has instituted a number of experiments which led to facts hitherto unknown; and as they are highly important, we shall exhibit the results of the principal ones, which are as follows:

* The practical mechanic may perhaps find it an advantage to be informed, that the whole effort of a first mover cannot be transmitted by this, or any other method of raising weights, in order to operate by their fall. If the wind had been employed to raise a maximum of weight through a given space in a given time, this weight would be less than would continue in equilibrio at rest against the same force, as is well known; and if this weight be again employed in like manner to raise another maximum of weight, this also will be less than the former, &c. For this reason it is that the fly has been used as an equalizer of steam-engines, in preference to the older method of raising water and suffering it to descend on an overshot wheel.

W. N.

† Abridged from a memoir in Gehlen's new Journal of Chemistry, Vol. I. Part IV. Page 406, by — A.

1. Prussic acid can only be formed during the carboniza- Prussic acid is formed only at red heat.
tion of blood or animal matter, *at a red heat*: We, therefore, need not be afraid of heating the mixture of blood and alkali, in the preparation of prussiated alkalies, to incandescence.

2. The affinity of prussic acid with alkalies is greater at Its affinity with alkalies is then greatest.
high temperatures, and in the *dry way*, than at low temperatures, and in the *humid way*.

This observation distinguishes prussic acid from the rest of All other vegetable acids are destroyed by ignition.
the so called animal and vegetable acids, and their combinations with alkaline bases; for all of them are destroyed at a red heat. Hence it is obvious,

3. That in the preparation of prussiated alkalies, the pre- Water should be avoided.
sence of water should be avoided as much as possible.

4. The direct combination of prussic acid with alkalies, can- P. acid and alk. do not directly combine.
not be accomplished.

5. Pure prussiated alkalies are decomposable by the affusion P. prussiated alk. decomposable by water.
of water; part of the prussic acid escapes, and may easily be recognised by the odour of bitter almonds, and frequently also by that of ammonia.

6. The combination of prussic acid with alkalies can only P. acid does not combine with alk. at low temp. but by medium of iron, &c.
be effected *at all temperatures* up to a red-heat, by the interposition of a portion of oxide of iron; and the affinities consisting between the prussic acid and alkali, are retained with a greater force in the humid way, in the ratio of the ponderable quantity of iron present.

7. All the precipitates obtained in chemical analysis by Common prussiates contaminate metals with iron; pure p. give a different result.
means of prussiated alkalies prepared in the usual manner, contain more or less iron, without exception; whereas the same precipitates, produced by the action of absolutely pure prussiates, are free from that metal, and of a different colour than the former.

8. The affinity of oxide of iron to charcoal, is more power- Most of the iron present remains in the coal, when p. alk. is made.
ful than the joint attraction of prussic acid and potash to that metal; hence we always find that the greatest quantity of that metal remains behind with the charcoal, in the usual process for obtaining prussiate of potash.

9. The precipitability of the oxides of metals by prussiated alkalies, is in the ratio of their oxidability, or quantity of Metals are precip. by p. alk. in the order of oxidation.
oxygen they contain.

Such are the observations of this chemist. Other less interesting facts will be omitted in the present abstract.

Experimental

Experimental Enquiries concerning the best Method of preparing Prussiate of Potash free from Iron.

Preparation of prussiate of alkali, pure.
Low ignition of dried blood and potash, and solution by water.

Four oz. dried blood were intimately mixt with a solution of potash (containing one ounce of potash), then evaporated to dryness, ignited to redness, till no more flame undulated at the surface of the ignited mass. The ignited mass was diffused through six ounces of water, and the solution filtered. The filtered fluid was colourless, it contained an excess of alkali, and emitted a strong odour of bitter almonds. It yielded, on being evaporated as expeditiously as possible, a saline mass, consisting of a mixture of prussiate and sub-carbonate of potash. In order to separate these two salts, one drachm of the saline mass was introduced into a vial containing a mixture of half an ounce of highly concentrated alcohol and half a drachm of acetic acid, of 1,056 spec. grav. On agitating the fluid no sensible effervescence took place, but much prussic acid was disengaged; a proof the prussic acid was retained with a less affinity by the alkali than the carbonic acid. On examining the residue, which had been treated with acetic acid and alcohol, it was found to contain only a small portion of prussiated alkali.

The salt was treated with acetic acid and alcohol.

Very little prussiate, and not separable from the carbonate by those agents.

From this experiment we learn, that one part of potash cannot be converted into prussiate of potash, by being heated to redness with four of blood; and that the quantity of either free or carbonated alkali, under these circumstances, cannot be separated from the prussiated potash by means of acetic acid and alcohol.

Less of blood was then used in the ignition.

In order to learn if a less quantity of blood would not be more advantageous for the production of pure prussiate of potash, four ounces of dried blood, and two of carbonate of potash, were heated to redness in a crucible till no more flame appeared. On covering the mixture with charcoal powder, and again heating it, a prodigious evolution of ammonia took place; a phenomenon I do not venture to explain. The mass, after having been diffused through water, filtered and evaporated, yielded a crop of crystals, consisting of prussiate and sub-carbonate of potash, the former predominating considerably.

Great evolution of ammonia.

The subcarbonate and prussiate not separable by alcohol and acetic acid.

On subjecting this mixture of salts to the joint action of acetic acid and alcohol, it was found impossible to separate the prussiate of potash from the sub-carbonate.

Being

Being thus persuaded that a less quantity of blood was not more advantageous for forming pure prussiate of potash, I again mingled two ounces of dried blood with one of carbonate of potash: this mixture was treated as before, with the exception that the mass, after the flame had entirely disappeared, was strongly ignited for three quarters of an hour. The obtained mass, after refrigeration, weighed nine drachms. Digested cold with four ounces of water, and filtered and evaporated, it afforded a dark coloured fluid. On dropping into a considerable quantity of muriatic acid, no blue, but a white precipitate fell down, which was insoluble in muriatic acid: Acetic acid occasioned no change in this dark-coloured fluid; and on mingling it with sulphuric acid, and evaporating the mixture to dryness, and re-dissolving the mass in water, it yielded a small quantity of prussian blue; a proof that iron was present in this fluid.

Blood and alkali, as in the first experiment, strongly ignited for three quarters of an hour.

The fluid of solution was dark, and contained some iron, though very little.

The quantity of iron being very small, but the colour of the fluid very dark when compared with those of the former processes, it was natural to suppose, that the colouring-matter could not be attributed to the minute quantity of iron present, but that perhaps a portion of charcoal was dissolved in the fluid: to investigate which the following experiments were instituted:

A like quantity and like proportions of blood and carbonate of potash, as stated last, were gradually heated to incandescence, and the fire gradually augmented, until the mass began to fuse on the sides of the crucible. The mass, after having been diffused through water and filtered, yielded a much darker-coloured fluid, which, when mingled with muriatic acid, yielded a pearl-coloured precipitate. After being mixed with muriatic or sulphuric acid, evaporated and redissolved in water, it afforded a considerable quantity of oxide of iron; a proof that the union of the prussic acid with potash is permanent at very high temperatures, but that this combination, under such circumstances, exercises a strong action upon the oxide of iron contained in the blood.

Repetition of the experiment with fusion of the mass.

The prussic acid remains united with alk. at very high temp. and takes iron strongly. Less oxide taken up by less heat.

These facts were proved by repeating this experiment; but taking care to expose the mass to a somewhat less degree of heat. The fluid now obtained was less coloured, and yielded less oxide of iron.

This

Experiments
with pure alkali.

This being proved, the author varied his experiments, so as to be thoroughly convinced of the facts. He also endeavoured to ascertain, whether alkalies, freed from carbonic acid, were better calculated for the production of prussiated alkali; and, if possible, to find out the proper proportions of ingredients for obtaining this salt. With that view, a quantity of solution of potash, containing one ounce of alkali freed from carbonic acid, was mixt with three ounces of blood, and evaporated to dryness. (It was of a colophony colour, soluble in water, and emitted, on being heated, a very strong odour of ammonia). On being transferred into a crucible, and gradually heated till no more flame appeared, and diffused through four ounces of water, it yielded a *limpid* fluid, of a strong alkaline taste, and odour of bitter almonds. The usual experiments proved, that it contained comparatively little prussiated alkali, but a large quantity of carbonate of potash.

Low heat produced little combination of p. acid and the alkali.

Exp. to determine the proportions of carbonate and blood requisite to produce the prussiate, &c.

The experiments with low heat.
4 oz. blood and
1 alk. carb.

Being thus convinced that this experiment proved fruitless, it was deemed necessary to ascertain the respective quantity of oxide of iron contained in the prussiated alkali, that might be produced from a given quantity of blood and carbonate of potash.

With that view, four ounces of dried blood were mixt with a solution of carbonate of potash, containing one ounce of carbonated alkali: the mixture, after being evaporated to dryness, was heated till the flame ceased to appear. It now weighed $1\frac{1}{2}$ oz. On being elixivated with eight ounces of water, it afforded a fluid of a very pale yellowish brown, or wine colour: Its taste was alkaline, mixt with that of bitter almonds. Two drachms of water mixt with 20 drops of a concentrated solution of muriate of iron, when decomposed by this prussiated alkali, afforded four grains of prussian blue, stript from the adhering oxide of iron by muriatic acid.

Product very little.

2 oz. blood,
1 carb. alk.
low heat.
Product not increased.

Two ounces of dried blood and one of carbonate of potash, treated in a similar manner, yielded, by being mingled with a like quantity of muriate of iron, $4\frac{1}{2}$ grains of pure prussian blue.

(The Remainder in our next.)

XVI.

*Observations on the Cause which augments the Intensity of the Sound in Speaking Trumpets. By J. H. HASSENFRATZ *.*

ALTHOUGH the speaking-trumpet is an instrument which Antiquity of the speaking trumpet. has been long known, since Kircher is of opinion that Alexander used it to command his army, and Solard made one in Paris, in 1654, from the description which Kircher had given of that of Alexander, it was not, however, until 1671, when Morland made known that which he had constructed, and invited men of science to determine the figure most proper for this instrument, that the speaking-trumpet was really known among us, and began to be used.

It appears that Morland did not adopt the conical form terminated by a mouth piece, which he gave to the speaking trumpets of glass, iron and copper, he made until after a succession of trials on the best form to be given to them, to make them produce articulate sounds.

It also appears that it was without knowing it, and by chance, Cassaigne's. that Cassaigne gave the speaking trumpet, which he made in 1672, the hyperboloidal form first noticed by Sturm.

Until 1719, when Haffé published a dissertation on the improvement of speaking trumpets, this instrument was constructed without principles; for we neither can nor ought to consider as principle, the harmonic proportion mentioned by Cassaigne as being necessary between the length and the width of the tubes of speaking trumpets. Were at first constructed without known principles.

The law of the reflection of sound in echoes, led Haffé to apply the theory of catoptrics to speaking trumpets, and induced him to consider the combination of the *ellipsoidal* and *paraboloidal* forms as the most advantageous for this instrument: Haffé's theory of the augmentation of the intensity of sound in them. but this union not producing the effect which the professor of Wirtemberg had hoped for, it was abandoned, and in conformity with his opinion, the augmentation in the intensity of the sound continued to be considered as the product of the reflection of the sonorous rays in the speaking trumpet, and of the vibration of the substance of which it was composed. It was for this reason that, in the fabrication of these instruments, the

* From *Annales de Chimie*, Praireal, An. XII.

attention was directed to having a hard, elastic and thin substance, that it might vibrate in unison with all the tones, and reflect the sonorous rays which struck it in all directions.

Lambert's theory.
Vibration injures the articulation of the sounds.

In 1763, Lambert withdrew from the theory of speaking trumpets, the vibration of the materials of which they are composed: he showed that the vibration of the sides which is calculated to augment the intensity of a long continued sound, would render articulate sounds confused, which must succeed each other rapidly; that in this case therefore it would be necessary to speak extremely slow, and that, even by speaking slowly, it would be impossible to distinguish the consonants, which are only momentary modifications of the vowels; that the latter, pronounced in the trumpet, would be so sonorous, that the consonants must be guessed at, which would be extremely difficult. By sprinkling the exterior surface of a tin speaking-trumpet with saw-dust, I satisfied myself that the surface vibrated in some circumstances, but I was equally satisfied, on covering the outside of the trumpet with a soft loose stuff to stop and obstruct its vibrations, that the intensity of the sound was no less strong, in this second case, and, that thus the vibration of the substance of the trumpet was, at least, useless, if it was not injurious to the distinctness of articulated sounds.

Speaking trumpets do vibrate, but the sound is equally strong when the vibration is prevented.

In his memoir, published among those of the Academy of Berlin, for the year 1763, Lambert has attributed all the augmentation of the intensity which the sound experiences by speaking into a trumpet, to the reflection of the sonorous rays on the smooth and polished surface of the interior of this instrument; he says that, of all the forms, that best calculated to concentrate the sound by reflecting it, and the most easy to construct, is the conic form: he afterwards shows that the sound is strengthened in conical speaking trumpets, in the proportion of double the length of the cone which forms the trumpet to double the size of half the angle at the summit of the cone, supposing the length of the cone to be equal to the radius. If the angle of the cone is made $= \phi$, the sound is strengthened in the proportion $\sqrt{2} : 2$ (sin. $\frac{1}{2} \phi$).

Proportions of a conical speaking trumpet.

Seeking, from these formulæ, to discover what would be the best proportions to give to conical speaking trumpets, he found that whatever the angle of the summit of the cone was, its base must be equal to the distance between the summit of the

the cone and the mouth of it, and that its length must be equal to the diameter of the mouth, divided by four times the square of the sine of half the angle of the cone: that thus in a trumpet of six feet in length, the angle of the cone must be $16^{\circ} 17'$, and that such a speaking-trumpet would make the sound 96 times more intense.

Lambert therefore, as well as those who have preceded him, considered the augmentation of the intensity of the sound in speaking trumpets, as the result of the action of the reflection of the sonorous rays against the interior sides of the instrument.

It would appear that since Lambert's memoir, speaking trumpets have been made use of, without attending to the causes which augment the intensity of the sound, and, in fact, it is seen that in all works in which this instrument is spoken of, its effects are attributed either to reflection alone, or to reflection combined with the vibration of the substance of which it is formed. These instruments have latterly been constructed without attending to the causes of the increase of sound.

If we compare the theory of speaking trumpets with that of the instruments which have some resemblance to it, such as trumpets and hunting horns, we must be astonished to find that they are referred to different principles. In the speaking trumpet, the cause is attributed to the reflection of the sound; in the other instruments to the vibration of the air contained in the tube. Why these two causes, while the effects are analogous? This analogy has led me to inquire if, in reality, the reflection produced the augmentation of the intensity of articulation in horns and acoustic tubes, as well as in speaking-trumpets, as is generally believed. Theory of speaking trumpets and other instruments. Does reflection increase the strength of sound?

On examining the ears of animals, it is seen that the greatest number have an exterior auricle, which is directed towards the place from which the sound proceeds; this auricle being, in many animals, covered inwardly with hair, which stops and hinders the reflection of the sound, it is reasonable to conclude that it is not by reflection that the sound is transmitted into the ear. Animals do not hear by the reflection of sound.

When afterwards we observe the form given to acoustic horns, which is that of a cone, the summit of which, slightly truncated, is placed in the ear, we are quickly led to conclude that the sound is not transmitted by reflection; for the angle of the incident ray being increased at each reflection by that of the cone, after a number of determinate reflections, the angle would Ear-trumpets do not transmit sound by reflection.

would become larger than a right angle, and the ray would return upon itself. Thus the greatest number of the rays would issue out of the mouth, after being several times reflected, and the quantity of the rays which thus reach the ear, and which are proportional by less numerous as the cone is longer, cannot sensibly augment the intensity of the sound. Nevertheless, acoustic horns do strengthen the sound considerably; this augmentation must therefore depend on another cause.

Horns of a paraboloid form disused.

It has been proposed to make horns of a paraboloidal form, as being best calculated to transmit sound by reflection; but whether they did not produce the effect which was expected from them, or that they were too difficult to construct, these horns have been abandoned, and none but conic horns are made use of.

Cylindrical acoustic tubes transmit sound without reflection to a great distance.

The acoustic tubes employed to transmit the sound to great distances, so that it cannot be heard by persons placed between the two extreme points, are usually formed of cylinders, which serve to conduct the sound. Lambert had already stated that reflection did not increase the intensity of sound in cylinders, because whatever might be the direction of the incident rays, being constantly reflected between two parallels, the angle which they form with the axis of the cylinder must be the same at entering and quitting it; that thus the sound must experience a diminution at its issue, which will be so much the greater as the number of reflections is more considerable. Nevertheless the sound is transmitted to a great distance by means of these tubes, and I have ascertained that the beating of my watch, which ceases to be audible at the distance of 1^m. 10, the medium of seven experiments (*A*), is heard at the distance of 2^m. 25, the mean distance of seven experiments (*A*) when I place it in the mouth of a pasteboard tube, 0^m. 038 in diameter and 0^m. 6 in length; whence it follows that this transmission must proceed from another cause.

According to the theory of reflection the enlarged part (pavillion) is of no use.

All these considerations have therefore induced me to examine the effects of the speaking trumpet with the greatest care. It follows from the theory of the reflection of sound, applied by Lambert to the speaking trumpet, that the opening by which it is usually terminated, should be at least useless, if not injurious, for it has no influence on the concentration of the reflected rays; and therefore this gentleman proposed to lay it aside entirely. The analogy of form between the trumpet

pet and the speaking trumpet not being a sufficient reason for retaining it, it is probable that it would have been laid aside, in the same manner as the contour of the trumpet was abandoned, when the form and dimensions most suitable to this instrument were sought for by experimental trials. There would also have been another inducement to dispense with it, because Haffé had already omitted it in the speaking trumpet which he indicated as preferable to those of Morland and Cassegrain. I ascertained by experiment that two speaking trumpets of the same length and the same diameter produced different effect, when they were terminated by an enlarged aperture or not, and that in general this enlargement considerably increased the intensity of the sound. The beating of my watch which I heard at 4^m. 2, the mean distance of seven experiments (*a*) by placing it at the mouth of a speaking trumpet of 0^m. 6 long and 0^m. 38, mean diameter of the tube, was not heard beyond 2^m. 25, mean distance of seven experiments (*a*) when I placed it in a tube of the same length and the same diameter. Thus the cause which occasions this aperture to strengthen the sound is different from the reflection of the sonorous rays.

Which is contrary to experiment.

Since the reflection of sound does not in any manner concentrate the sonorous rays in a cylindrical tube, it must follow, as Lambert has concluded in his Theory of Reflection, that speaking trumpets with cylindrical tubes should not sensibly augment the sound. To prove whether experiment agreed with this theory, I immediately constructed a speaking trumpet, with a cylindrical tube of 0^m. 36, the length of the cylinder, 0^m. 25, the length of the enlarged part, 0^m. 035, the diameter of the cylinder, and 0^m. 190, the greatest diameter of the enlarged part, and, by comparing the intensity of the sound which it produced, with that of a conical speaking trumpet of the same height, and of a similar mean diameter, I ascertained that the strength of the sound was sensibly the same. In both, the beating of my watch, which I did not hear in the open air beyond 1^m. 08, mean distance of five experiments (*b*) was heard at 5^m. 94, mean distance of five experiments (*b*) when I placed it at the mouth of either speaking trumpet. Since the cylindrical speaking trumpet strengthens the sound in the same manner as that which is conical, and since, on the contrary, the theory of reflection shows that it should not be strengthened, it follows that the augmentation of sound in these instruments arises from a cause different from reflection.

This is further proved by the sound being augmented in speaking trumpets with cylindrical tubes, as strongly as in those of a conical form.

The intensity of the sound is not lessened by destroying the reflection by means of an internal woollen covering.

Except inasmuch as it diminishes the diameter of the instrument.

The diminution in the intensity of the sound is compensated by its clearness.

General conclusions.

One decisive experiment remained to be made with these instruments, this was to destroy and to restore the action of the reflection in a speaking trumpet, supposing it to take place, and to compare the intensity of the sound obtained in these two circumstances, in order to determine the proportion of the effect produced by this reflection. I consequently procured a speaking trumpet of woollen stuff, which I placed within one of tin, and I observed that the intensity of the sound obtained with or without the stuff in the inside, was sensibly the same; whence it follows that the reflection has no appreciable influence on the augmentation of sound in a speaking trumpet.

I have said that the intensity of the sound was sensibly the same; nevertheless I must observe that it was diminished; for the beating of my watch, placed in the mouth of the bare speaking trumpet was heard at a distance of 3^m. 94, mean of five experiments (*b*), while when placed in the mouth of the same instrument covered in the inside with the stuff, it was not heard beyond 2^m. 48. This difference, which at the moment might be supposed to arise from the reflection of the sound taking place in the one case, and not in the other, seems to depend solely on the diminution of the diameter of the cylinder of the speaking trumpet and of the enlarged parts occasioned by the stuff placed within them: I satisfied myself that the speaking-trumpet which I made use of (*b*), preserving all its dimensions and experiencing no difference except in the diameter of the tube, produced a diminution in the intensity of the sound, whenever a smaller diameter was made use of; thus, in the speaking trumpet already mentioned (*b*), the beating of my watch was heard at a distance of 3^m. 94, when the diameter of the tube was 0^m. 035; it was not heard beyond 3^m. 08, when the tube was only 0^m. 032; finally, it was not heard beyond 2^m. 34, when the tube was 0^m. 028. It is evident, therefore from these experiments, that the diminution in the intensity of the sound by covering the interior of a speaking trumpet with stuffs, arises from the diminution of the diameter of its tube.

I should observe that if the internal stuff occasions an inconsiderable diminution in the intensity of the sound, it, on the other hand, produces a great advantage by rendering the articulated sounds clearer and less confused.

It follows from all which has been shewn on the auricles of animals, on horns, and acoustic tubes, and on speaking trumpets,

pets, that the augmentation of sound, in every circumstance, is not owing to the reflection of sonorous rays, consequently there exists no reason for separating the cause which augments the sound in these instruments, from that which strengthens it in trumpets and hunting horns; that the different sounds produced in the two cases are owing to the vibration of the air in the tubes, and their strengths or their intensities, to the augmentation of the amplitude of their vibration, arising from the greater impulse which the air necessarily receives when it is enclosed in a tube.

Experiment (A).

The tubes and the speaking trumpet of pasteboard.

The interior diameter of the tube of the speaking trumpet is 0^m. 038, that of the exterior diameter of the enlarged part = 0^m. 210. The length of the tube = 0^m. 4, and the length of the enlarged part = 0^m. 2.

	With a tube of the length of			The speaking trumpet.	Without tube or speaking trumpet	Observations.
	m. 0.4	m. 0.6	m. 0.10			
Distance at which the beating of the watch was heard.	1.33	1.66	2.33	4.00	1.00	The great difference between these experiments arises, 1 st , from the disposition of the organ;—2 ^d , from the greater or less noise produced in the neighbourhood of the place where the experiments were made.
	2.24	2.90	3.33	4.66	1.16	
	2.16	2.66	3.33	5.00	1.33	
	1.33	1.79	1.85	3.00	0.83	
	1.92	1.95	2.00	5.00	1.33	
	1.58	2.16	2.66	3.66	1.00	
	2.54	2.66	3.17	4.00	1.00	
	12.86	15.78	18.67	29.33	7.65	
Mean	1.83	2.25	2.67	4.20	1.10	

Experiment (B).

The speaking trumpet was of tin, as well as the tubes.

The interior diameter of the tube of the speaking trumpet = 0^m. 035, that of the exterior diameter of the enlarged part = 0^m. 290, the length of the tube = 0^m. 36, and that of the enlarged part = 0.25.

	With an internal diameter of			Without speaking trumpet or tube.
	m. 0.035	m. 0.032	m. 0.028	
Distance at which the beating of the watch was heard.	4.88	3.58	2.76	m. 1.48
	3.58	2.93	2.28	0.97
	3.25	2.76	2.28	1.30
	4.39	3.58	2.44	0.82
	3.58	2.58	1.95	0.82
Sum	19.78	15.43	11.71	5.39
Mean	3.94	3.08	2.34	1.08

XVII.

*Account of Cerium, a new Metal found in a mineral Substance from Bastnas, in Sweden. By W. D'HESINGER and J. B. BERGELIUS *.*

I.

Description of the Tungstein of Bastnas.

Discovery of new substance in tungstein.

ALTHOUGH this substance has been formerly assayed by Scheele and D'Elhuyar, under the name of wolfram, its considerable weight, nevertheless, determined us to submit it to farther enquiries. Our principal object was to find yttria in it, which, being unknown at the time in which these chemists operated, might have escaped their attention. Our supposition was not well founded, since, instead of an earth, we discovered a substance which, according to every appearance, is hitherto unknown, as will be seen in the sequel.

Where found.

The tungstein of Bastnas, which we call *cerite*, for reasons which will be presently given, was found, in the year 1750, in a copper-mine called Bastnas, or Saint-Gorans Koppargruva, at Riddare-Hyltan, in Westmannia, of which, with *asbestos*, it formed the matrix: but after this time it was imbedded in quartz and mica, to the depth of seventeen toises.

* From the Swedish, by G. A. Linbom; but here translated from the *Annales de Chimie*, L. 145.

The tungstein is almost always mechanically mixed with Description of
black amphibole (*hornblende*), striated actinote, of a clear the ore.
green colour (*short*), mica, sulphurated copper, bismuth, and
sulphurated molybdena, which may be easily known by ex-
posing it to the fire.

The cerite, properly so called, is transparent, of a flesh Physical properties of cerite,
colour, sometimes deep and sometimes bright, seldom yellow-
ish. In a mass, or in small specimens, the stone is of an ir-
regular form; its fracture is indeterminate, compact, and a
little brilliant, with obtuse edges; its consistence is tenacious
and strong; it gives fire with steel with difficulty, but does
not scratch glass; it is not attracted by the magnet; after
having been made red-hot in the fire, it loses its hardness,
and six or seven *per cent.* of its weight: by this operation it
becomes friable, and acquires a bright yellow colour: it does
not melt alone.

On account of its weight, Cronstedt has placed it among Is the false tung-
the tungsteins, in his Mineralogy. In pure fragments, it is stein of Scheele,
to that of water as 4.733 and 4.935 to 1.000. Scheele not
having found wolfram in it, called it false tungstein.

The constituent principles of this mineral were given by Constituent
Bergman, in the Memoirs of the Academy for the year 1784, principles.
page 121, from an analysis of D'Elhuyar: they are as fol-
lows:—

Silex,	-	-	-	0.22
Iron,	-	-	-	0.24
Lime,	-	-	-	0.54
				<hr/>
				1.00

Heated with borax by the blow-pipe, it forms a globule of Habitudo with
glass, which, while hot, appears greenish, but is colourless borax.
when cold. Urged with carbonate of soda in a platina spoon
it is not dissolved.

§ II.

Analysis of the Proportions of Cerium.

TO separate the yttria which was supposed to be in it, it Treatment of
was reduced to a fine powder in a porphyry mortar; pure the ore with al-
concentrated nitric acid was then poured upon it. The acid tric acid, &c.
was decomposed, and a considerable quantity of nitrous gas
and

and carbonic acid gas were disengaged. The stony powder was repeatedly treated with this acid, until the insoluble residue appeared white.

The solution diluted with water was of a yellow colour, which became greenish by boiling, and afterwards red: completely dried, it became of a yellowish white, but regained its red colour by attracting humidity. It is entirely dissolved in alcohol; and the solution, slightly digested, deposits a considerable quantity of oxide of iron. It likewise deposits more oxide of iron by remaining for some days undisturbed in a window. The decanted solution, being almost clear, was evaporated to ficcidity, and the calcined salt was in the form of a powder, of the colour of bricks. Water could only dissolve the calcareous earth. Distilled vinegar could only take up a very small portion, and was not saturated, though assisted by the heat of ebullition. The evaporated acetic solution gave small granulated crystals, of a saccharine astringent taste. They were not totally soluble in alcohol. The part of the acetous salt which was not dissolved in alcohol, gave, by calcination, a brick-coloured powder, resembling that which had not been dissolved.

Indications of a
metallic sub-
stance.

Ammonia precipitated the alcoholic solution in a white powder, which became yellowish in the air. It was a little soluble by carbonate of ammonia, and acquired the colour of bricks by calcination. The sediment being separated, the carbonate of ammonia produced a white precipitate, which was pure carbonate of lime. The acetous salt did not therefore contain yttria. The powder from which the calcareous earth had been separated, dissolved in muriatic acid, with a disengagement of oxygenated muriatic acid gas, which indicated that there was a metallic oxide.

Was it oxide of manganese combined with oxide of iron?

To ascertain this, we endeavoured to develop the pure oxide of manganese by means of tartrate of potash, according to Richter's method. We decomposed in this manner, a solution of this substance in muriatic acid, perfectly neutralized by tartrate of potash; and after having washed the precipitate well, we submitted it to a slow calcination; but it only produced the brick-coloured powder.

Does not contain
alumine.

Caustic alkali had no action on the insoluble part of the nitrate; which proves that it did not contain alumine.

To

To obtain the pure metallic matter in a sufficient quantity to make several assays, another portion of cerite was dissolved in nitric acid, and the solution evaporated to siccity. Water was poured on the residue, and it was precipitated with ammonia. The washed precipitate was dissolved in nitric acid. The solution, well neutralized with the alkali, was afterwards precipitated by tartrate of potash. A white powder was also precipitated from the same solution by carbonate of potash, but it was in small quantity. These precipitates were separately calcined, and both of them acquired the colour of bricks. The precipitate formed by the carbonate of potash, was not dissolved by potash aided by digestion; it therefore did not contain alumine. The iron contained in the solution, precipitated with tartrate of potash, was separated by hydro-sulphuret of ammonia. The remainder of the solution of cerite in nitric acid, which had been precipitated by caustic ammonia, gave carbonate of lime by carbonate of ammonia.

From these assays it results, that cerite contains nearly 23 parts of silice, 5.5 of carbonate of lime, 22 of oxide of iron, and a quantity of this metallic matter, the weight of which, after calcination, rather exceeded 50 per cent. But this substance being then, as well as the iron, united with more oxygen than they contained in the cerite, we have, instead of a loss, an augmentation of weight, which probably arises from the oxygen. Neither is the loss which the cerite experienced in the calcination, included in this account. We also found traces of manganese, but in so small a quantity, that potash, melted with the cerite and dissolved in water, did not give any colour.

Not having the practice which complete proportionate analyses require, we offer these results with diffidence, and in the hope that scientific men of more experience will employ themselves on this subject.

§ III.

Examination of the Metallic Oxide found in the Cerite.

A PASTE was made of 37 grains of this oxide and linseed oil, which was reduced in charcoal in a covered crucible. Although it retained some carbon, it lost half a grain of its weight. This mass was inclosed in a lined crucible without

out any flux, and M. Hjelm exposed it for half an hour to the degree of fire necessary for the reduction of manganese. The oxide was not melted, but reduced into a very fine powder: it exhibited brilliant particles in the day-light, and stained white paper black. It dissolved in muriatic acid, disengaging, at the first, sulphurated hydrogen gas, and afterwards pure hydrogen gas. This colourless solution had a saccharine taste. Thus it appeared to us that the metal was reduced in part. The origin of the sulphur may be traced to the sulphuric acid, from which the matter had been separated by the caustic ammonia. The influence which this acid exercises in these assays, will be seen by the subsequent enquiries.

M. Gahn, at Fahlun, having more convenient furnaces, has promised to undertake the reduction of this substance with more power: if this operation succeed, we shall give an account of it hereafter.

It is the oxide of a metal not yet known.

These appearances, and those which follow, determined us to consider the substance found in the cerite, as the oxide of a metal hitherto unknown, to which we have given the name of Cerium, from the planet Ceres, discovered by Piazzi.

Manner of obtaining the pure Oxide of Cerium.

Processes for obtaining the pure oxide.

(A.) Pure uncalcined cerite was dissolved in nitro-muriatic, and, after saturating the clear solution with the alkali, was precipitated by tartrate of potash. The precipitate well washed, calcined, and digested in vinegar, contains the pure oxide of cerium.

Or otherwise decompose a solution of cerium in nitro-muriatic acid, still warm, but not saturated, by succinate of ammonia: a succinate of iron is gradually deposited. The precipitation is to be continued by means of succinate of ammonia, as long as a red precipitate is formed: the solution is then nearly deprived of iron. After having separated the succinate of iron, more succinate of ammonia is poured into it, until a white precipitate appears. The solution is then left at rest, in order that the small portion of succinate of cerium may be deposited. The iron dissolved by the free muriatic acid, is deposited at the same time, and the solution is freed from this metal. The cerium may afterwards be precipitated by ammonia, and then washed and calcined.

Of the Properties of Oxide of Cerium.

(B.) This oxide may appear in different degrees of oxidation. The alkalis precipitate a white oxide from its solutions, which shows of a yellowish colour in the air, but, when perfectly dried, becomes dark. Exposed to a brisk and long-continued fire, it takes a deep brick-colour. The oxalate and acetate of cerium, calcined in vessels not completely closed, yield a white oxide, which, in an open fire, becomes of the colour of bricks. It does not melt by itself. The oxide is capable of different degrees of per-oxidation.

Treated with borax by the blow-pipe, it melts readily and swells. The globule heated by the exterior flame, assumes the colour of blood; which, by cooling, passes to a yellowish green, and at length becomes colourless, and perfectly transparent. Melted by the interior flame, these changes do not take place; it is then reduced into a colourless glass; but, exposed for a short time in the exterior flame, the same phenomena are produced. If too much oxide of cerium is made use of, the glass resembles an opaque yellowish enamel. These changes are more easily manifested with the phosphate of soda and ammonia. If two clear and colourless globules are melted together, one of which was prepared with borax and the other with the phosphate, they form a transparent glass, which, on cooling, becomes opaque and pearl coloured. Fusion with fluxes.

These characters, taken together, sufficiently distinguish the oxide of cerium from the oxide of iron. The latter also offers the same changes of colour, but its glass, on cooling, has a deep green colour, which fades. The globules made with borax and the phosphate melted together, yield an opaque glass, the colour of which is a little deeper.

Oxide of Cerium treated with Sulphuric Acid.

(C.) When oxide of cerium is digested with sulphuric acid, these two substances unite, and the result is a red insoluble salt, which is sulphate of cerium at a maximum of oxidation. Sulphate of cerium at a maximum of oxidation. If the acid is concentrated, it scarcely dissolves any of it. If it is diluted with half its quantity of water, or a little more, the result is a yellowish oily liquor, which does not adhere to the glass, nor does it wet it. If the acid is mixed with six or seven times its quantity, or even more, of water, and employed in a sufficient quantity, the oxide is dissolved of an orange

orange colour. By a slight evaporation of this solution, it yields small, prismatic, coherent crystals, of the colour of gold. This salt is an acidulous sulphate of cerium at a *maximum*. These crystals, thoroughly dried between blotting-paper, and exposed to the air, are gradually reduced to a yellow, almost crystalline powder. Redissolved in water, they experienced a decomposition; a white powder is deposited, and the solution becomes colourless. This white powder is a sulphate of cerium, but little oxygenated. If the solution is evaporated to dryness, it gives an acidulous sulphate of cerium still less oxygenated. These crystals are seldom cubical, but almost always prismatic, striated and collected in bundles. Their taste is sour, but they afterwards become saccharine and astringent.

Neutral acid.

Acidulous sulphate of cerium but slightly oxygenated.

Muriatic acid deprives the acidulous sulphate of part of its oxygen: As does an increase of temperature, which afterwards drives off the acid, and forms a neutral sulphate. Calcination restores the oxygen.

Action of the alkalis on the sulphates of cerium.

Sulphate of cerium and potash.

Treated with muriatic acid, the yellow acidulous sulphate of cerium yields part of its oxygen to the acid, which is volatilized in oxygenated muriatic acid gas. The salt remains colourless. An augmentation of temperature alone is sufficient for the yellow acidulous sulphate of cerium to lose its colour by losing the excess of its oxygen. If the heat is increased still more, the surplus of the acid is carried off, and a saturated sulphate of cerium remains. By a continued calcination, it regains oxygen, becomes red, and yields a sulphate of cerium at a *maximum*. The sulphate of cerium, difoxygenated by the muriatic acid, is more difficult to re-oxidate by calcination.

In the humid way, the alkalis only decompose the sulphate of cerium incompletely. Ammonia precipitates an oxide from the acidulous sulphate of cerium, which is only in a small quantity, but nevertheless contains part of the sulphuric acid. The sulphate of cerium is not perfectly decomposed, except by calcination with three times its weight of carbonate of soda or potash. The calcined oxide is of a brown colour. By digestion, ammonia can deprive it of part of its acid: the oxide takes a distinct flesh colour, which becomes brighter by drying. Digested with concentrated muriatic acid, or with nitric acid, a small quantity dissolves, having its yellow colour.

If a solution of acidulous sulphate of cerium is precipitated by potash, a triple combination of cerium, sulphuric acid, and potash, is separated, before the acid is saturated. If too much potash is added, the combination is partly destroyed. The sulphate of cerium and potash, at a *maximum*, is of an orange colour;

colour; that which is at a *minimum*, is white. A similar combination is also obtained by pouring muriate of cerium into a solution of sulphate of potash. Sulphate of ammonia does not form any precipitate in it; but, on adding a calcareous salt to it, sulphate of cerium combined with potash is rapidly deposited.

These characters offer a ready method of separating the sulphate of cerium from iron. It must however be observed, ^{Sulphuric acid in excess frees sulphate of cerium from iron.} that when the solutions are saturated, a little iron is also deposited, which gives a yellow colour to the precipitate; but by adding a little sulphuric acid in excess, the iron is redissolved, and leaves the precipitate entirely white. This combination is only dissolved in part by dilute sulphuric acid, and the greatest quantity of that separates afterwards.

The sulphate of cerium and potash melts by a strong heat. ^{Sulphate, and} ~~Feated~~ with charcoal, it gives sulphuret of potash and sulphuret of cerium. ^{Melted with carbonate of potash, in closed carbonate of ce-} vessels, it yields carbonate of cerium and sulphate of potash. ^{rium.} This salt contains only one-third of oxide of cerium.

It is dissolved in concentrated nitric acid, and, during the ^{Concentrated} cooling, an acid salt, formed of acidulous sulphate of potash ^{nitric acid de-} and a little sulphate of cerium, crystallizes. Thus the sulphate ^{composes sul-} of cerium combined with potash, is decomposed by concen- ^{phate of cerium} trated nitric acid. This acid carries off the metal, and the sulphuric acid is directed wholly upon the potash, with which it forms a salt with excess of acid.

Oxide of Cerium with Nitric Acid.

(D.) Nitric acid dissolves the calcined oxide with difficulty, ^{Nitrate of cerium.} but that which is precipitated by pure or carbonated alkalies, with ease. When the solution is saturated with oxygen, it is of a greenish yellow colour; but colourless, when less oxidized. Evaporated to the consistence of honey, it deposits lamellated crystals, which attract the humidity of the air. The solution has a saccharine taste: like all the other saturated solutions of cerium, it lets fall an oxide of cerium, at a *maximum* of oxidation, in the open air. This precipitate is frequently formed of oxide of iron. When dry, this salt is of a yellowish white colour; but becomes colourless on being dissolved in a sufficient quantity of water. It dissolves readily in alcohol.

A concentrated solution of this salt takes a blood colour, on account of a small quantity of iron, which, by drying, passes to yellowish white, but is restored by a new solution.

A colourless and less oxidized nitrate of cerium, is obtained by dissolving the yellowish salt in alcohol: the solution inflames and yields a white salt.

It is destroyed by fire, which drives off its acid.

With Muriatic Acid.

Muriate of cerium.

(E.) The calcined oxide of cerium is slowly dissolved in muriatic acid in the cold, and more readily by heat; an effervescence is produced, owing to a disengagement of oxygenated muriatic acid gas. The taste of the solution is saccharine and astringent; the colour is a very faint greenish yellow: the dried saline mass is yellowish white, and attracts humidity. We only succeeded once in obtaining it crystallized. The crystals were white, brilliant, in four sided prisms, with the points cut off. The salt dissolves readily in alcohol, and its concentrated solution burns with a yellow and sparkling flame. The residue of the salt is white and gives a colourless solution. It is muriate of cerium at a *minimum* of oxidation.

Heated in closed vessels, the water of crystallization is first dissipated, afterwards the acid passes in the form of oxygenated muriatic acid gas. If the operation is stopped before the acid is entirely volatilized, an undecomposed muriate of cerium, at a *minimum* of oxidation remains.

If the muriate of cerium contains iron, it all sublimes in a brown deliquescent mass. Nothing remains in the matrass but a white oxide of cerium, which attracts the humidity of the air, and becomes yellow. Thus, sublimation with muriate of ammonia may be employed to purify a muriate of cerium which contains a little iron.

With Phosphoric Acid.

Phosphate of cerium.

(F.) Free phosphoric acid, saturated with an alkali, precipitates muriate of cerium. The precipitate is white, and soluble in muriatic acid and in nitric acid employed in sufficient quantity.

This salt is also obtained by digesting pure oxide of cerium, moistened with phosphoric acid. It is not soluble in an excess of this acid.

With

With Carbonic Acid.

(G.) The carbonate of ammonia precipitates muriate of Cerium ^{Carbonate of cerium} without effervescence. After the precipitation, carbonic acid is slowly disengaged in the form of bubbles. The residue retains its acid, even after desiccation.

Dry carbonate of cerium has a white colour tending a little to bluish or greenish. It dissolves in the acids with effervescence. It does not lose its acid in an open fire. In closed vessels, without the contact of oxygen, it supports a gentle calcination, without being decomposed.

With Arsenic Acid.

(H.) Free arsenic acid does not produce any change on Arseniate and muriate of cerium. The oxide digested with this acid, forms ^{acidulous arseniate of cerium.} an insoluble salt. An excess of this acid dissolves it, and gives an acidulous arseniate of cerium. The saturated arseniate of cerium is deposited in the form of a powder during the evaporation. The residue does not crystallize, but by desiccation, becomes a gelatinous, clear, and colourless mass.

With Molybdic Acid.

(I.) The acidulous salts of cerium are not decomposed by Molybdate of cerium ^{molybdate of ammoniac.} The molybdate of cerium is precipitated from its saturated solutions, in the form of a white salt, which is not soluble in the acids.

With Oxalic Acid.

(K.) Either the acidulous or saturated solutions of cerium ^{Oxalate of cerium.} are precipitated by oxalic acid. According to the degree of oxidation of the metal, the precipitate becomes red or white. This combination is also obtained by digesting the oxide with free oxalic acid. An excess of acid does not dissolve it, but ammonia readily effects its solution, giving it a yellow colour.

A small quantity of oxide is deposited by evaporation. The solution afterwards yields regular crystals in the form of needles. Pure alkalis do not occasion any precipitate.

With

With Tartareous Acid.

Tartrite of cerium.

(L.) Free tartareous acid has no action on muriate of cerium. The recently precipitated oxide unites with this acid by digestion, and yields a tartrite of cerium, which dissolves readily in water. This salt is also precipitated from saturated solutions by tartrite of potash. Like the oxalate of cerium, it dissolves in pure ammonia, but does not crystallize.

Tartrite of cerium is not entirely soluble in water; the solution is precipitated by carbonate of soda.

With Benzoic Acid.

Benzoate of cerium.

(M.) This acid does not act on the muriate of cerium; but, by digestion, well concentrated benzoic acid dissolves the oxide of cerium recently precipitated. On cooling, the solution first deposits crystals of the acid in excess, and afterwards benzoate of cerium in the form of a white powder, which adheres to the crystals of benzoic acid.

The resinous matter with which this acid is frequently united, combines with the benzoate of cerium, and forms an insoluble brown powder.

With Citric Acid.

Citrate and acidulous citrate of cerium.

(N.) Muriate of cerium is not precipitated by citric acid. But the oxide digested with citric acid forms an insoluble saturated combination, which an excess of acid dissolves. The acidulous citrate of cerium is of a yellow colour and does not crystallize. Alcohol deprives it of its water, and of part of its acid, but does not dissolve it.

With Acetic Acid.

Acetate of cerium.

(O.) The calcined oxide of cerium is only very imperfectly dissolved in acetic acid, even with the assistance of heat; but that which is recently precipitated by the alkalis, is dissolved with facility. The saturated acetate of cerium is soluble in water; it has a sweet taste, and gives granulated crystals, by evaporation, which do not change in the air, and are but slightly soluble in alcohol.

This salt swells in the fire, and is destroyed.

(To be continued.)

Dutch proof, from five pounds of the dried fruit. This brandy, although it retains a slight odour of the fruit, is by no means disagreeable to the taste, and I have made liqueurs with it in no respect inferior to those of commerce. I shall not delay the publication of the details of this discovery, from which I claim no credit except on account of the influence which it may have on a part of the commerce of the eastern provinces of Spain.

*Method of obtaining pure Cobalt. By TROMSDORFF *.*

LET four parts of finely pulverized zaffre be intimately blended with one of very dry nitrate of potash and half a part of charcoal powder; introduce this mixture by small quantities at a time into an ignited crucible, and repeat this process for three successive times, by again adding to the detonated residuary mass new quantities of nitrate of potash and charcoal. This being done, mix the mass with one part of black flux, and expose the mixture to a red heat for one hour. When cooled, separate the metallic cobalt, pulverise it, blend it together with three times its own weight of nitrate of potash, and detonate this mixture as directed before. The iron which was present will thus become highly oxidized, and the arsenic acidified, and combined with the potash. To separate the latter, pulverise the mass, treat it repeatedly in water, and separate the insoluble part by the filtre. The arseniate of potash being thus got rid of, digest the residue in nitric acid; the cobalt will now be dissolved, and the highly oxidized iron remain untouched. Evaporate the solution to dryness, redissolve it in nitric acid, re-filtre the solution, in case some oxide of iron should have escaped unseparated, decompose the solution of nitrate of cobalt by potash, wash the precipitate, and reduce it by heat.

Method of coating Copper with Platina. By STRAUSS.

Mr. Strauss, who has made a number of good experiments on platina, has succeeded in applying this metal to defend the face of copper. The solution of platina was precipitated by

Coating copper
with platina.

* Gehlen's Journal of Chemistry, Vol. IV. p. 6, 117.

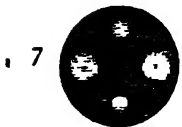
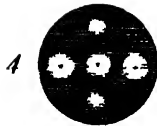
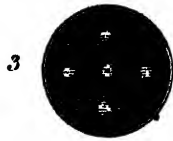
muriate of ammonia, then washed and dried, and exposed to a graduated red heat for half an hour in a covered crucible. The product was a grey coagulated powder; consisting of metallic platina in a state of extreme division. One part of this powder with three parts of mercury did not combine by half an hour's trituration; but upon adding two parts more of mercury and slightly heating the mortar, he soon obtained a tenacious amalgam, which was rendered very soft by the addition of two other parts of the last mentioned metal.

A small quantity of this amalgam was rubbed upon a plate of copper, which became perfectly covered. The plate was then ignited, and was found to have retained a coating of platina. In the next place he mixed a little of the amalgam with chalk, sprinkled the mixture with water, coated the plate of copper a second time, and again ignited it. The coating was now found to be very perfect, and assumed a shining silver colour under the burnisher.

This chemist remarks that his application of platina to copper vessels must be superior to that of tin; not only in its resistance to acids and saline matters, but in its durability from the greater hardness of platina; and he adds that the process here described, is not more difficult to be effected than the common operation of tinning.

Tromsdorff's Journal, 1803, Vol. II. p. 18.

Tubular Pendulum by W. Edward Thompson.



Fig

2



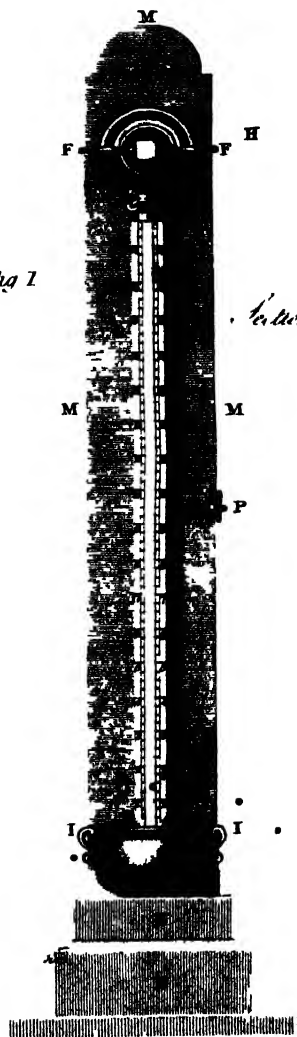
Fig

1



*Instrument for measuring the Absorption of the Gases
by Charcoal by C. L. Berzelius*

Fig 1



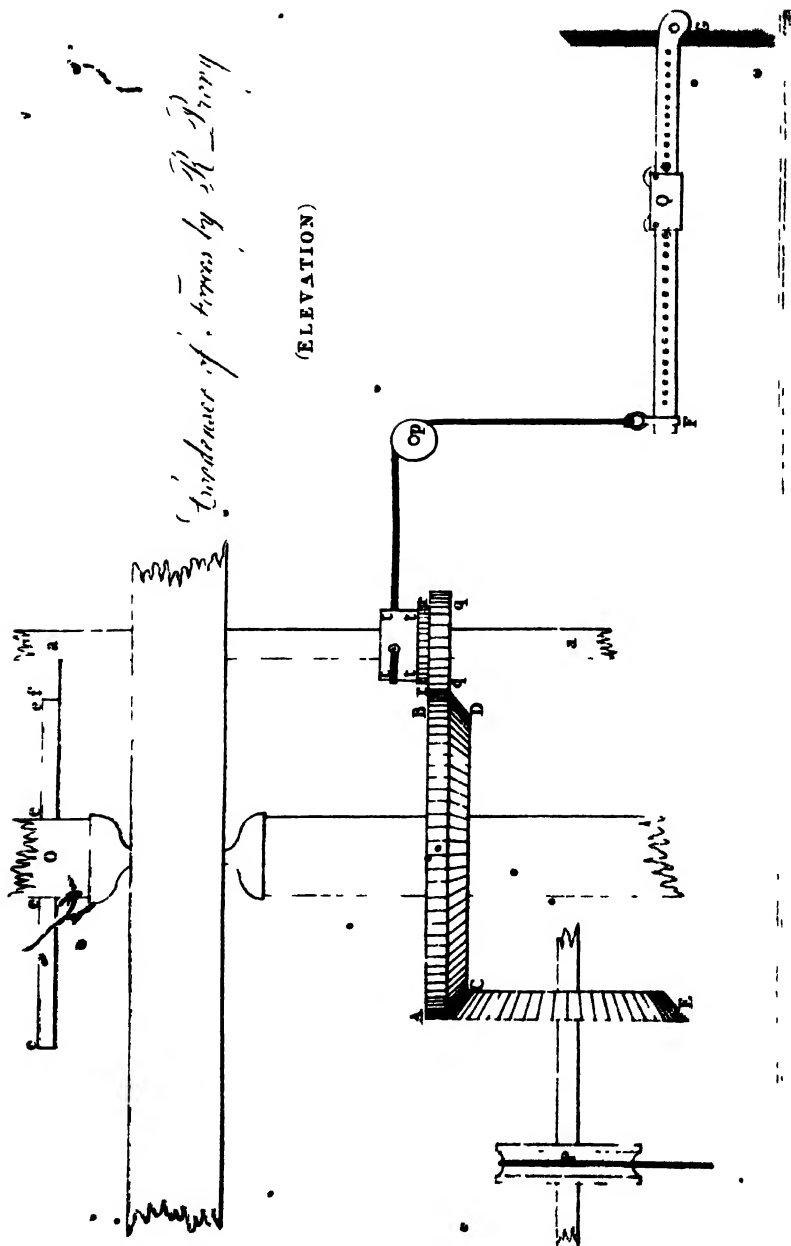
Section in the Line AB



Fig 3

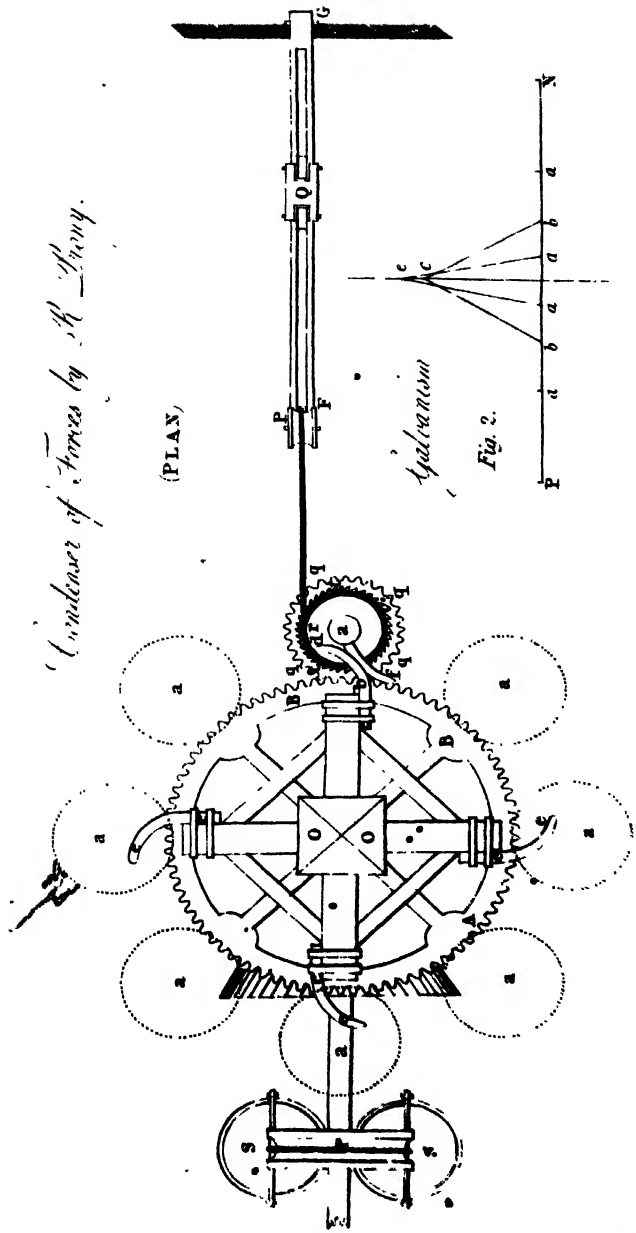


Fig 2



Condenser of Forces by H. Henry.

(PLAN.)



Hydrovacuum

Fig. 2.

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END OF THE NINTH VOLUME.

ERRATUM.

In *Plate XII. Fig. 3.*, by an omission in the design, the lower ruler has not the same connection with the frame as the upper. It must be joined by prolonging the rod from the left hand angle, and adding a rod from thence to the bar upon the ruler; as is shown above.

